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THE PSYCHOLOGICAL REVIEW

THE EFFECT OF TIME-INTERVAL UPON RECOGNITION MEMORY[1]

BY EDWARD K. STRONG, JR.

Columbia University

In the November issue of the PSYCHOLOGICAL REVIEW it was shown that the per cent. of advertisements that can be correctly recognized decreases as the length of the series is increased.[2] The plotted results showed a slight curve—almost a straight line. Increasing the difficulty of the task by adding more stimuli results then in a slow decrease in the ability to identify the previously shown advertisements. The purpose of this paper is to study the effect produced upon recognition memory when a different sort of difficulty is introduced into the operation. In this case the same number of stimuli is used throughout the study but the intervals between exposure and identification are varied. Here we find no sort of an approximation to a direct proportion between the efficiency of identification and the varying intervals of time. Instead, we find a very rapid decrease in efficiency up to about 2 hours after exposure. From that point on the decrease in efficiency becomes more and more gradual until it is apparent that there is little or no difference between the effect of an interval of 7 days and that of 42 days upon recognition memory.

It is not necessary to point out here the relationship between the results of this experiment and those of other workers.

[1] I wish here to express my sincere thanks to Dr. and Mrs. H. L. Hollingworth, to Mrs. Clara Hart, and to my wife, for having served as subjects in this experiment, and especially to the last, who also served as coexperimenter.

[2] E. K. Strong, Jr., 'The Effect of Length of Series upon Recognition Memory,' PSYCH. REV., XIX., 6, November, 1912.

A recent monograph[1] covers this field satisfactorily in that it compares the curves of forgetting for different materials and different methods of investigation. But it is somewhat of a help to an appreciation of our results to state at the start that there is a very close parallel between these results and those given us by Ebbinghaus in his study of recall memory.[2]

Reference to Plate III. will show at a glance the relationship between the two studies.

The Experiment

The experiment was as follows. From the Standard Dictionary were taken all the ordinary common words,—with few exceptions none over three syllables in length was used. Each word was written on a slip of paper. After all had been thoroughly shuffled they were divided into two boxes,—one used by subject *A* when she acted as experimenter and the other used by subject *B* when he acted as experimenter. A list of 40 words drawn at random from the box was first prepared. These 40 were then shuffled together and another list of 20 was made from these 40. Here some guidance was exercised in order that words which obviously made sense should be widely separated. But in the main, even these lists were formed by chance. This second list of 20 words was the exposure list. The first list of 40 words was given to the subject after the designated interval of time had elapsed and from it he was expected to choose those words which had been in the first list.

[1] C. H. Bean, 'The Curve of Forgetting,' *Archives of Psychol.*, No. 21, 1912. In this monograph, which appeared in the midst of our experiments, a forgetting curve, for the 'method of selection' was given. This is the same method as employed here and called 'recognition memory.' The form of Bean's curve would probably be identical with ours if his method of scoring had been adequate. But he overlooked the fact that chance selections had to be reckoned with in the scoring. In his experiments 9 consonants were exposed and later had to be selected from a total of 18 consonants. Each correct choice of the 9 consonants was scored 1/9 of 100 per cent. But random chance would give a selection of 4½ correct and 4½ incorrect or a score of 50 per cent. according to his method of scoring. In the same way he counted up the number of substitutions but there again it would be impossible to get a lower score than 50 per cent. Just what his actual scores should be cannot be determined for lack of the detailed scores.

[2] H. Ebbinghaus, 'Ueber das Gedachtniss.' Leipzig, 1885.

The exposure lists of 20 words were written on slips of paper, the words appearing in a column, one under the other. The subject was instructed to read the words aloud at his leisure. He was told that he might read them slowly enough to actually grasp the meaning or content of the word, but must read them fast enough not to be able to *consciously* form associations between them. Both *A* and *B* found at the start that reading aloud was a decided help in aiding them to refrain from forming associations between the successive words. There was no intention on the part of the experimenter that the subjects should not form associations of any sort here, but merely that they should not consciously attempt to form them. Subject *D*, alone, found difficulty in inhibiting the conscious attempt to form associations between the words in the list. But this was only at the start. After a few trials he overcame the tendency to build pictures out of the words as he found it was a serious handicap when it came to recognizing the separate words later. The aim throughout was to obtain a maximum of attention upon 20 separate words, not as brute syllables but as words with meaning. A constant rate of exposure for each word would have given a uniformity of exposure when considered in terms of physical time. But this does not mean that there would have been necessarily in the exposure a uniformity in the psychological processes of perception and attention. In fact, some experiments of the writer, not yet reported, show very clearly that when a number of stimuli are being presented in succession at a constant rate, the attention devoted to each varies enormously. Process of interference of some sort probably accounts for this. To repeat then, the aim was to obtain as nearly as possible the maximum attention upon each word. From a careful study of the results—both statistical and introspective—we feel sure that the aim was obtained in the great majority of cases.

At the bottom of each of the exposure lists was a short problem in mental arithmetic. The subject was required upon finishing reading the list immediately to solve the problem. This device was necessary in order to prevent the seemingly instinctive tendency to run the eyes back up the column in

order to grasp the last four or five words and hold on to them. It was a rare exception that the subject would recall any of the 20 words within a few minutes after the exposure. Record was kept of the words that were recalled when the recognitions were made but they were so few in number and so scattered that no statistical treatment of them could be made. This seems rather surprising, but it should be borne in mind that the subjects did not read the lists with the intention of remembering the words so as to be able to recall them later. Our "mind-set" or attitude toward the task before us has seemingly everything to do with the way in which we "take-in" stimuli.[1]

One objection to the use of common words as material in such a study must be recognized. Such words do vary in the ease with which they will impress any given mind. But as several thousand different stimuli had to be employed it seemed to the experimenter that words or numbers were the only possible material that could be employed. Naturally words were chosen. The different parts of speech vary also in their strength of impressing themselves on the mind. It was a mistake to employ prepositions, conjunctions, and pronouns. As it turned out they were more difficult to remember than the other parts of speech. They could have been omitted as there were only a few of them. The ideal thing would be to use only nouns, but there are not enough of them for the purposes of this study. The only effect of using these different parts of speech has been to cut down the reliability of the results.

When the second list of 40 words was given, the subject was expected to check those words in the list which he remembered as having seen in the exposure list of 20 words. The following procedure was followed in the checking. A "1" was placed after those words which the subject was *absolutely* sure were in the exposure list (100 per cent. sure); a "2" was placed after those words which he was *reasonably* sure were in the exposure list (about 75 per cent. sure); a "3" after those words which he had a faint idea were in the list (about 25 per cent. sure); and a "4" after those words which were a pure guess.

[1] For a very extreme example of this, see H. L. Hollingworth, 'The Influence of Caffeine on Mental and Motor Efficiency," *Archives of Psychol.*, No. 22, 1912, p. 17.

TABLE I

SHOWING A SPECIMEN RECORD SHEET (THE FOURTH RECORD OF SUBJECT *A* FOR THE INTERVAL OF 4 DAYS)

The words with numerals before them comprised the exposure list. The numerals placed after the words indicate *A*'s recognitions and their degree of certainty.

	timid	4
6	scour	
	vanilla	
	thirty	
	wane	4
	this	
	saddle	
	tremble	
	favor	
	progress	
	thin	
	obstinate	
	persistent	4
7	industry	4
16	oblong	4
	zoölogy	
3	witch	1
13	ink	
	owl	4
19	gradation	1
	scatter	
4	founder	2
17	quinsy	1
5	then	
14	massacre	2
12	warmth	4
	scorn	
	victor	4
11	reveal	3
	week	
2	faculty	1
1	militia	3
15	permit	
	wash	
20	traitor	2
9	resume	
	quill	3
8	wharf	
10	percolate	1
18	mask	3

Summary

Pile No. 1, 5 correct, 0 incorrect.
Pile No. 2, 3 correct, 0 incorrect.
Pile No. 3, 3 correct, 1 incorrect.
Pile No. 4, 3 correct, 5 incorrect.

A "4" was necessary because the subject was required to check 20 words each time. The five subjects all understood these

requirements thoroughly. The four degrees of certainty will be referred to respectively as Piles No. 1, No. 2, No. 3, and No. 4. In the scoring, Pile No. 2 is credited at ¾ of the value of Pile No. 1, Pile No. 3 at ¼ value, and Pile No. 4 at 0 value. This system of scoring is fair, because it was on the basis of such a scoring that the subjects assigned the various recognitions to the four piles.

Table I. gives a typical record sheet. This is the fourth record of subject *A* for the interval of 4 days. Before 20 of the words appear numerals from 1 to 20. They represent the order of these 20 words as they appeared in the exposure list. That is, the words with a numeral before them were in the exposure list and should have all been checked with a "1" to give a perfect recognition record. After the words in the list appear the 4 numerals referring to the four degrees of certainty of the recognitions. At the right of the list appears a summary. We see then that 5 words placed in Pile No. 1 were correct and 0 were incorrect; 3 words placed in Pile No. 2 were correct and 0 incorrect, 3 were correct and 1 incorrect in Pile No. 3, and 3 correct and 5 incorrect in Pile No. 4.

Thirteen different intervals of time were regularly used in the experiment. They were "immediately after exposure" or 15 seconds, 5 minutes, 15 minutes, 30 minutes, 1 hour, 2 hours, 4 hours, 8 hours, 12 hours, 1 day, 2 days, 4 days, and 7 days. One complete record consists of an experiment with each one of these intervals. As it turned out fewer intervals could have been used just as well. But in view of the Ebbinghaus curve, it was thought that it would be well to have records for short intervals after the exposure. In addition to the above thirteen intervals, two records were obtained, one each from *C* and *D*, for an interval of 42 days. The order of experimenting on the thirteen intervals was determined the first time by chance. Thereafter, the order was arranged so that any one interval would come equally often at the commencement, in the middle, and at the end of the orders. In this way the advantage due to practice effect was evenly divided among the thirteen intervals. Only one experiment was carried on at a time on any one subject. When that was completed, the

TABLE II

SHOWING THE PER CENT. OF CORRECT RECOGNITIONS FOR EACH OF THE THIRTEEN INTERVALS STUDIED

(The four degrees of certainty of the recognitions are separately noted for each of the five subjects and their summary)

Interval Studied	*A* (Av. 5 Records)				*B* (Av. 5 Records)				*C* (Av. 2 Records)				*D* (Av. 2 Records)				*E* (1 Record)				Summary (Av. 15 Records)			
	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4
15 sec...	92	0	3	0	71	12	6	0	93	0	0	0	93	0	2	0	80	0	0	0	**84.3**	**4.0**	**3.3**	**0**
5 min..	73	6	7	0	68	9	7	1	88	5	0	2	75	8	2	0	65	0	5	5	**73.0**	**6.7**	**5.3**	**1.0**
15 min..	76	5	5	0	61	9	8	2	58	13	10	0	40	15	5	15	45	15	5	0	**61.7**	**9.3**	**6.7**	**2.7**
30 min..	68	6	8	0	53	6	17	5	53	5	5	15	63	8	7	2	40	0	10	5	**58.3**	**5.7**	**10.7**	**4.3**
1 hr. ..	69	5	9	0	48	11	15	3	58	26	0	2	43	10	7	2	50	5	5	10	**55.7**	**10.3**	**9.3**	**2.3**
2 hrs. .	62	4	8	1	43	12	19	3	53	5	0	5	43	18	7	5	40	10	5	0	**50.3**	**9.0**	**10.3**	**2.7**
4 hrs. .	61	5	15	1	34	19	20	0	40	10	13	5	45	15	10	8	55	15	5	0	**46.7**	**12.3**	**15.0**	**2.0**
8 hrs. .	52	6	18	0	34	11	19	5	35	18	13	10	35	20	8	8	35	0	10	20	**40.3**	**10.7**	**15.7**	**5.3**
12 hrs. .	48	14	17	0	24	17	17	6	43	21	5	5	50	5	13	7	30	5	15	15	**38.3**	**14.0**	**14.7**	**4.7**
1 day..	25	18	23	0	25	11	18	6	55	10	8	10	25	15	2	13	25	5	5	20	**29.0**	**13.3**	**15.3**	**6.3**
2 days	25	15	25	1	19	13	24	8	35	25	2	2	28	10	7	18	15	0	35	15	**24.0**	**14.0**	**20.0**	**6.7**
4 days.	21	11	34	3	14	10	36	1	18	18	12	12	15	5	23	15	40	5	0	15	**18.7**	**10.3**	**28.0**	**6.0**
7 days.	16	11	30	6	9	10	26	8	0	30	10	10	5	12	7	33	20	15	10	15	**10.3**	**13.7**	**21.7**	**11.3**
42 days.	—	—	—	—	—	—	—	—	0	15	10	35	0	0	20	35	—	—	—	—	**0**	**7.5**	**15.0**	**35.0**

TABLE III

SHOWING THE PER CENT. OF INCORRECT RECOGNITIONS FOR EACH OF THE THIRTEEN INTERVALS STUDIED

(The four degrees of certainty of the recognitions are separately noted for each of the five subjects and their summary)

Interval Studied	*A* (Av. 5 Records)				*B* (Av. 5 Records)				*C* (Av. 2 Records)				*D* (Av. 2 Records)				*E* (1 Record)				Summary (Av. 15 Records)			
	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4	Pile 1	Pile 2	Pile 3	Pile 4
15 sec...	0	1	4	0	0	3	8	0	0	2	0	5	3	0	0	2	0	5	10	5	0.3	2.0	4.7	1.3
5 min..	1	1	11	1	4	1	9	1	0	0	5	0	2	0	5	8	5	0	5	15	2.3	0.7	8.3	2.7
15 min..	1	2	11	0	1	1	17	1	2	10	2	5	0	7	5	13	0	5	25	5	1.0	3.7	12.0	3.0
30 min..	5	3	9	1	0	2	13	4	2	2	8	10	0	8	7	5	5	20	15	5	2.3	4.3	10.3	4.0
1 hr. . .	4	4	8	1	1	0	16	6	2	2	2	8	0	7	18	13	0	5	5	20	2.0	3.0	11.0	6.3
2 hrs. .	2	3	18	2	5	5	19	4	0	15	12	10	0	7	10	10	10	10	5	20	3.0	6.3	12.3	6.0
4 hrs...	1	1	15	1	0	9	18	0	5	10	10	7	0	0	2	20	5	0	10	10	1.3	4.7	13.3	4.7
8 hrs...	1	3	20	0	3	6	17	5	2	7	5	10	2	2	10	15	5	10	5	15	2.3	5.0	14.7	6.0
12 hrs...	3	5	13	0	2	4	24	6	2	10	7	7	0	2	0	23	0	5	10	20	2.0	5.0	14.0	7.3
1 day..	4	3	27	0	4	7	27	2	0	7	8	2	0	5	7	33	5	20	5	15	3.0	6.3	20.3	6.3
2 days.	5	9	17	3	3	6	18	9	5	23	0	8	2	5	18	12	0	0	20	15	3.7	8.7	15.3	7.7
4 days.	3	5	18	5	2	6	26	5	2	15	8	15	0	7	15	20	10	0	15	15	2.7	6.7	18.7	9.0
7 days.	3	6	24	4	2	11	30	4	0	23	20	7	0	10	15	18	5	5	10	20	2.0	10.3	23.3	7.3
42 days.	—	—	—	—	—	—	—	—	0	0	20	20	0	0	5	40	—	—	—	—	0	0	12.5	30.0

TABLE IV

VALIDITY OF THE RECOGNITIONS FOR EACH OF THE FOUR DEGREES OF CERTAINTY

Interval	Pile No. 1	Pile No. 2	Pile No. 3	Pile No. 4
15 sec.	99.6	66.7	41.3	0
5 min.	96.9	90.6	38.9	27.0
15 min.	98.4	71.5	35.8	47.4
30 min.	96.2	57.0	51.0	51.8
1 hr.	96.5	77.4	45.8	26.7
2 hrs.	94.4	58.8	45.6	31.0
4 hrs.	97.0	72.4	53.0	29.9
8 hrs.	94.6	68.1	51.6	46.9
12 hrs.	95.0	73.7	51.2	39.2
1 day	90.6	67.8	42.9	50.0
2 days	86.6	61.7	56.6	46.5
4 days	87.4	60.6	59.9	40.0
7 days	83.7	57.1	48.2	60.8

experiment on the next interval was commenced. The period of experimentation lasted from the first of January to the twenty-third of August, 1912. Because of the difficulty of fitting in several of the intervals and also because of the inability to complete the experiment due to interruptions or the failure on the part of the experimenter to remember to test the subject at the given hour, many of the tests which were planned for one interval had to be completed for a different interval. This in no way injured the experiment. On the other hand, it entirely prevented any unconscious behavior on the part of the experimenter from being communicated to the subject as to the length of interval then being tested, for not even the experimenter was sure that the interval he was planning to test would really be the interval that was eventually tested.

Five subjects were employed in this experiment, respectively, *A*, *B*, *C*, *D*, and *E*. The first four were experienced subjects, the fifth, the mother of *A*, was a novice. Five complete records were obtained from *A* and *B*, two complete records from *C* and *D*, and one record from *E*. Thus fifteen complete records for each interval were obtained. *A* and *B* were the experimenters: *A* experimented upon *B* and *C*, while *B* experimented upon *A*, *D*, and *E*.

THE RESULTS

1. *Correct Recognitions.*—Table II. presents the data concerning the correct recognitions. The average results for each

TABLE V

SHOWING THE EFFECT OF VARIOUS INTERVALS OF TIME BETWEEN EXPOSURE AND IDENTIFICATION UPON RECOGNITION MEMORY

The records for the five subjects are given separately and then a summary of the five. (Under each of the six divisions the results from each of the three degrees of certainty are noted together with a summary of the three different degrees of certainty. These summaries represent the score from Pile No. 1 plus ¾ the score in Pile No. 2. plus ¼ the score in Pile No. 3.)

	A (Av. 5 Records)					*B* (Av. 5 Records)					*C* (Av. 2 Records)				
	Pile No. 1	Pile No. 2	Pile No. 3	Summary	P.E.	Pile No. 1	Pile No. 2	Pile No. 3	Summary	P.E.	Pile No. 1	Pile No. 2	Pile No. 3	Summary	P.E.
15 sec.	92.0	−1.0	−1.5	**90.9**	*3.2*	71.0	8.6	−2.3	**76.9**	*2.1*	92.5	−2.5	0	**90.6**	*.4*
5 min.	71.2	5.0	−4.0	**73.9**	*5.2*	60.8	7.7	−1.3	**66.3**	*5.4*	87.5	5.0	− 5.0	**90.0**	*5.6*
15 min.	74.1	2.7	−4.2	**75.1**	*1.8*	59.2	7.4	−6.3	**63.1**	*3.7*	53.1	1.7	7.5	**56.2**	*12.8*
30 min.	60.1	3.0	−1.4	**62.1**	*5.8*	53.0	3.6	4.4	**56.8**	*6.7*	47.9	2.5	− 2.5	**49.1**	*3.5*
1 hr.	62.0	1.0	1.2	**63.0**	*3.4*	46.1	11.0	1.5	**54.8**	*1.8*	52.9	21.0	− 2.5	**68.0**	*2.4*
2 hrs.	58.3	1.0	−7.9	**57.1**	*4.7*	36.3	6.6	8.0	**43.3**	*6.5*	52.0	−8.3	−12.5	**42.6**	*4.9*
4 hrs.	59.2	4.0	.2	**62.3**	*6.6*	34.0	10.1	1.0	**41.8**	*5.9*	32.2	0	2.3	**32.8**	*6.9*
8 hrs.	50.2	2.5	−1.2	**51.8**	*4.2*	29.6	4.1	1.5	**33.1**	*5.9*	31.3	10.0	6.5	**40.4**	*19.3*
12 hrs.	42.6	8.1	4.5	**49.8**	*6.6*	20.9	11.4	−4.8	**28.3**	*3.6*	38.0	7.4	− 1.7	**43.1**	*0*
1 day	19.1	13.8	−2.5	**28.9**	*4.6*	19.6	4.3	−5.8	**21.4**	*5.6*	55.0	1.5	0	**56.1**	*.8*
2 days	17.5	4.2	6.3	**22.3**	*3.5*	15.6	5.7	4.8	**21.1**	*2.6*	27.9	1.4	2.5	**29.5**	*10.0*
4 days	16.2	5.2	11.1	**22.9**	*3.4*	11.6	3.2	7.9	**16.0**	*2.0*	13.8	2.9	3.8	**16.9**	*.2*
7 days	11.3	3.9	3.4	**15.1**	*2.7*	6.4	.5	−3.3	**6.0**	*3.2*	0	11.6	− 8.0	**6.7**	*6.4*
42 days	—	—	—	—	—	—	—	—	—	—	0	15.0	− 6.6	**9.6**	—[1]

	D (Av. 2 Records)					*E* (1 Record)					Totals (Av. 15 Records)				
	Pile No. 1	Pile No. 2	Pile No. 3	Summary	P.E.	Pile No. 1	Pile No. 2	Pile No. 3	Summary	P.E.	Pile No. 1	Pile No. 2	Pile No. 3	Summary	P.E.
15 sec.	87.8	0	2.5	**88.4**	*4.7*	80.0	− 5.0	−10.0	**73.8**	—[1]	83.7	1.9	−1.6	**84.6**	*1.9*
5 min.	70.3	7.5	−1.7	**75.5**	*4.6*	55.7	0	0	**55.7**	—	68.8	5.9	−2.7	**72.7**	*3.0*
15 min.	40.0	7.5	− .8	**45.4**	*4.0*	45.0	7.5	−16.6	**46.5**	—	59.8	5.1	−3.7	**62.7**	*3.0*
30 min.	62.5	0	0	**62.5**	*4.5*	31.1	−20.0	− 3.0	**15.4**	—	54.5	1.3	.5	**55.5**	*3.4*
1 hr.	42.5	2.1	−7.1	**42.3**	*5.0*	50.0	0	0	**50.0**	—	52.1	7.1	− .4	**57.3**	*2.0*
2 hrs.	42.5	8.1	−1.5	**48.2**	*8.6*	24.0	0	0	**24.0**	—	45.8	2.5	−1.8	**47.2**	*3.1*
4 hrs.	45.0	15.0	7.5	**58.1**	*4.9*	45.9	15.0	− 3.4	**56.3**	—	44.4	7.7	1.5	**50.6**	*3.4*
8 hrs.	30.6	16.3	−1.5	**42.5**	*6.3*	26.3	−10.0	3.4	**19.6**	—	36.6	5.0	1.0	**40.6**	*3.6*
12 hrs.	50.0	1.5	12.5	**54.2**	*1.1*	30.0	0	3.0	**30.8**	—	34.9	7.7	1.6	**41.1**	*2.7*
1 day	25.0	8.3	−3.8	**30.3**	*1.3*	16.7	−12.0	0	**7.7**	—	24.7	6.6	−3.3	**28.8**	*3.2*
2 days	23.0	5.0	−8.1	**24.7**	*.8*	15.0	0	9.6	**17.4**	—	18.8	4.2	3.6	**22.9**	*1.8*
4 days	15.0	−2.5	5.0	**14.4**	*2.6*	32.0	5.0	−15.0	**32.0**	—	15.2	3.2	6.5	**19.3**	*1.6*
7 days	5.0	1.5	−8.5	**4.0**	*1.1*	12.0	7.5	0	**17.7**	—	7.4	3.7	−2.2	**9.6**	*2.0*
42 days	0	0	12.0	**3.0**	—[1]	—	—	—	—	—	0	7.5	2.7	**6.3**	*2.0*

[1] Only one record.

TABLE VI

[1]Only one record.

TABLE VI

SHOWING PRACTICE EFFECT

The results of subject *A* are given in detail together with summaries of the five subjects

	First Record					Second Record					Third Record				
	Pile No. 1	Pile No. 2	Pile No. 3	Pile No. 4	Score	Pile No. 1	Pile No. 2	Pile No. 3	Pile No. 4	Score	Pile No. 1	Pile No. 2	Pile No. 3	Pile No. 4	Score
15 sec...	15–0	0–1	3–1		73.2	18–0	0–0	0–2		87.5	19–0	0–0	0–1		93.8
5 min..	12–0	2–0	3–3		67.5	11–1	0–0	4–4		45.9	17–0	1–0	0–1	0–1	87.5
15 min..	15–0	0–0	2–3		74.3	14–0	2–0	1–3		75.6	17–1	0–1	1–0		73.0
30 min..	12–4	2–0	1–1		37.5	13–1	1–0	2–3		58.7	10–0	1–1	5–3		51.6
1 hr...	14–2	2–0	1–1		60.0	13–2	0–1	2–2		43.7	14–0	0–0	2–4		68.4
2 hrs..	14–1	0–2	0–3		49.4	11–1	0–0	2–6		42.1	10–0	1–1	3–5		48.5
4 hrs..	8–1	1–1	6–3		33.6	10–0	0–0	5–5		50.0	15–0	2–0	1–0	1–1	83.8
8 hrs..	9–1	2–0	3–5		42.0	8–0	0–0	9–3		45.7	9–0	0–1	4–6		39.8
12 hrs..	7–1	3–2	4–3		32.1	8–1	1–1	6–3		33.6	8–0	1–1	5–5		40.0
1 day .	5–2	4–2	3–4		15.0	6–2	3–1	4–4		20.7	2–0	3–0	7–8		20.6
2 days.	7–2	2–2	3–4		18.7	4–2	1–1	9–3		12.3	2–0	3–2	7–6		13.0
4 days.	4–1	2–2	7–4		14.4	3–1	1–1	8–6		8.9	4–0	2–1	10–3		29.2
7 days.	5–1	3–1	6–4		23.8	3–0	0–4	7–6		0.7	3–1	1–0	8–7	0–0	11.9
Av. *A*..	9.8–1.2	1.8–1.0	3.2–3.0		41.6	9.4–0.8	0.7–0.7	4.5–3.9		40.4	10.0–0.2	1.2–0.6	4.1–3.8	0.1–0.2	50.9
Av. *B*..	6.9–1.4	3.2–2.1	2.6–3.8		27.3	6.0–0.1	2.5–0.9	4.8–6.0		33.5	6.8–0.2	2.1–0.6	4.9–3.5	0.8–1.1	40.8
Av. *C*..	9.8–0.4	2.8–1.0	1.2–1.2	1.4–2.2	52.1	9.5–0.4	2.8–2.9	1.2–1.5	1.1–0.8	43.7					
Av. *D*..	8.5–0.1	2.5–1.1	1.4–1.0	1.8–2.7	45.7	8.7–0.2	1.8–0.8	1.8–1.5	2.0–3.2	45.2					
Av. *E*..	8.3–0.8	1.2–1.3	1.7–2.2	1.8–2.8	44.2										

	Fourth Record					Fifth Record				
	Pile No. 1	Pile No. 2	Pile No. 3	Pile No. 4	Score	Pile No. 1	Pile No. 2	Pile No. 3	Pile No. 4	Score
15 sec...	20–0	0–0	0–0	0–0	100.0	20–0	0–0	0–0	0–0	100.0
5 min..	15–0	2–0	0–3	0–0	78.8	18–0	1–1	0–0	0–0	90.0
15 min..	17–0	1–0	0–2	0–0	86.3	13–0	2–1	1–3	0–0	66.1
30 min..	18–0	0–0	0–1	0–1	88.8	15–0	2–2	0–1	0–0	73.8
1 hr...	13–0	3–3	1–0	0–0	66.3	15–0	0–0	3–1	0–1	76.9
2 hrs..	12–0	2–0	1–2	1–2	66.7	15–0	1–0	2–2	0–0	78.8
4 hrs..	16–0	1–0	1–2	0–0	83.0	12–0	1–0	2–5	0–0	61.1
8 hrs..	12–0	1–1	1–5	0–0	55.9	14–0	3–1	1–1	0–0	75.7
12 hrs..	13–1	3–0	1–2	0–0	66.0	12–0	6–1	1–0	0–0	77.3
1 day .	7–0	2–0	6–5	0–0	43.2	5–0	6–0	3–6	0–0	45.0
2 days.	5–1	4–1	4–1	1–3	28.7	7–0	5–3	2–3	0–0	39.0
4 days.	5–0	3–0	3–1	3–5	38.2	5–1	3–1	6–4	0–0	23.8
7 days.	2–0	3–0	4–3	4–4	22.0	3–1	4–1	5–4	2–0	17.2
Av. *A*..	11.9–0.2	1.9–0.4	1.7–2.1	0.7–1.2	63.4	11.8–0.2	2.6–0.8	2.0–2.3	0.2–0.1	63.4
Av. *B*..	9.3–0.2	1.4–0.5	2.9–2.6	1.6–1.5	49.1	9.7–0.2	2.3–0.6	2.8–2.1	1.2–1.1	52.9

of the five subjects are shown. These results are stated in terms of per cent. of the total number of recognitions. That is, for subject *A*, out of 100 recognitions made immediately after exposure 92 were correct in Pile No. 1 and 3 were correct in Pile No. 3, while there was 1 incorrect recognition in Pile No. 2 and 4 in Pile No. 3 (see Table III.). (The details from which the records of Subject *A* in this table are made up appear in Table VI.) In the sixth division of the table is similarly presented a summary of the 15 complete records. The sum-

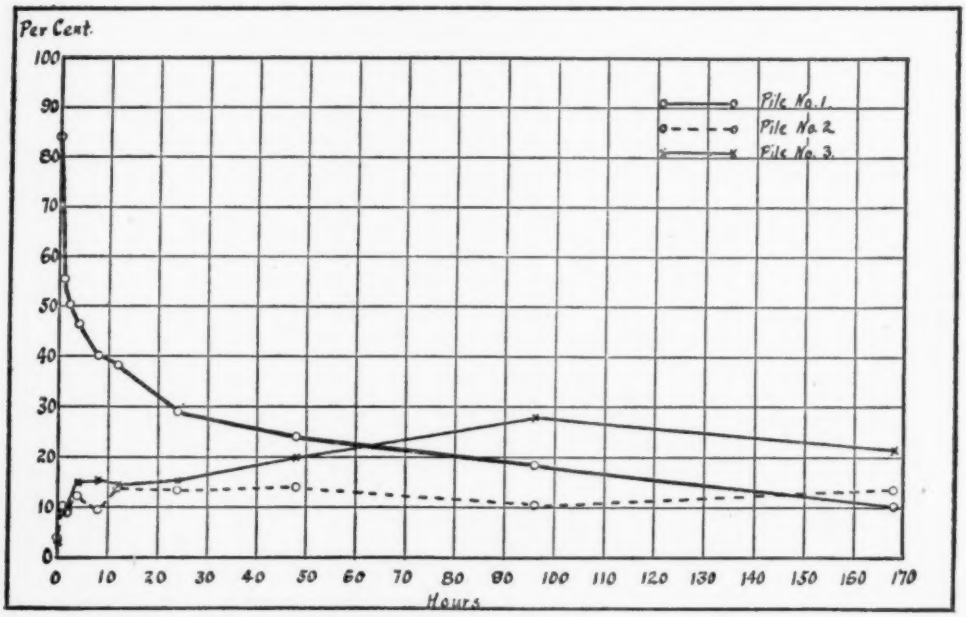

PLATE I. Showing the Effect Made upon the Per Cent. of Correct Recognitions by Lengthening the Interval between Exposure and Identification.

maries of Piles No. 1, No. 2, and No. 3 are shown in Plate I. From the table and plate it is very apparent that the per cent. of correct recognitions in Pile No. 1 drops very rapidly from "immediately after exposure" until an interval of 1 day. From there on the decrease is very much more gradual. The number of recognitions, which one is "reasonably sure" are correct, remains fairly constant. On the other hand, the per cent. of correct recognitions that are made in Pile No. 3 in-

creases from "immediately after exposure" to an interval of 7 days. This is due primarily to the fact that there are very few or no recognitions of the certainty of which the subject doubts, for the very short intervals of time. But as the interval of time increases the feeling of certainty accompanying the recognitions changes, becoming less and less positive. Only the first 3 degrees of certainty were employed with subjects *A* and B until the middle of the third series of experiments. Up to that time these subjects were forced to pick out words

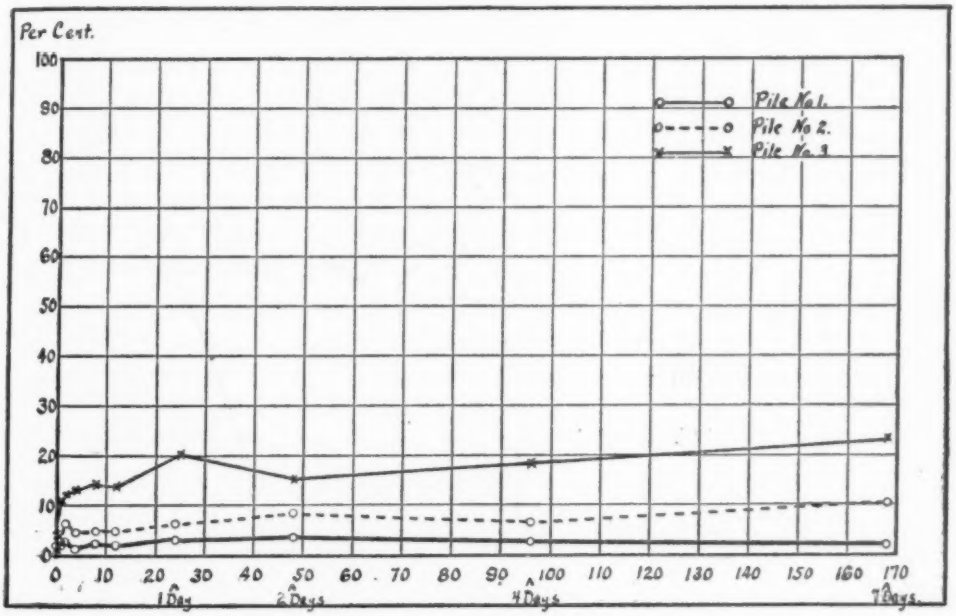

PLATE II. Showing the Effect Made upon the Per Cent. of Incorrect Recognitions by Lengthening the Interval between Exposure and Identification.

and put them in the third pile even when they felt that they were mere guesses. (As a matter of fact, very few such cases appeared.) After the change, such guesses were, of course, put in Pile No. 4. But neither of these subjects ever felt much need for this pile anyway. Subjects *C*, *D*, and *E*, however, used this fourth pile a great deal, as is shown in the table, and especially is this so in the case of the longer intervals.

Seemingly, some recognitions, which would have been put

in Pile No. 1 with a short interval, are put in Pile No. 2 with a somewhat longer interval, and with a very much longer interval of time between exposure and identification they are put in Pile No. 3. A study of the introspections apparently confirms this tendency. Pile No. 2 then constantly gained from Pile No. 1 and lost to Pile No. 3. The total number in the pile in this way remained practically constant.

Increase of difficulty in recognition, whether produced by increasing the number of stimuli presented at exposure or by increasing the interval between exposure and identification, causes first of all a pronounced decrease in the certainty with which the recognitions are made.

2. *Incorrect Recognitions.*—Table III. and Plate II. present the data with regard to the incorrect recognitions that were made in these experiments. Here we find that there is only a slight tendency to increase the total number of incorrect recognitions as the difficulty of the task is increased. There is actually an increase in this respect in all the three degrees of certainty. But the surprising thing is that it is so slight. It is evident here just as it was in the study of recognition memory with varying lengths of series that *an increase in the difficulty of the task does not cause one to make many more incorrect recognitions.* There is a limit, however, to the accuracy of this statement, as has been shown by R. H. Paynter, 3d, of Columbia University. In an unpublished study, he exposed pages of quarter-page advertisements at the rate of 1 page per 2 seconds. In this case there was a noticeable number of mistaken recognitions, especially with his subjects under 16 years of age. Nevertheless, within seemingly quite extended bounds, an increase in the difficulty of the task does not cause one to make many more incorrect recognitions.

The very much larger per cent. of incorrect recognitions in Pile No. 3 in the cases of subjects *A* and *B* than with the other three subjects is due largely to the fact, previously pointed out, that they did not have the use of a fourth pile, in which to put their mere guesses until half way through the entire experiment. If the experiment had been conducted with them at the start as it was at the end it is quite reasonable to suppose that

they would not have recorded against them such a large number of both correct and incorrect recognitions in Pile No. 3. Many of these would have been placed in Pile No. 4. This would have resulted in a lower per cent. of correct and incorrect recognitions in the summaries for the group.

3. *The Validity of the Recognitions.*—We have seen now that the correct recognitions in Pile No. 1 decrease very surprisingly in number as the interval of time is increased and that the correct recognitions in Pile No. 2 remain about constant in number, while those in Pile No. 3 increase in number as the interval is increased. We have also traced the tendency of the incorrect recognitions to increase in number with the increase of difficulty of the task, although this increase is comparatively slight. It is worth while to stop a moment to study the general validity of the recognitions for each pile under the varying conditions of the experiment. In other words, what per cent. of all the recognitions placed in Pile No. 1 were correct for the different intervals under study?

Table IV. expresses these relationships. Each figure in the table gives the percentage of correct recognitions from the total number of correct and incorrect recognitions in that situation. For example, 99.6 per cent. of all recognitions "immediately after exposure" which are felt to be absolutely correct are actually correct. This per cent. drops to 83.7 per cent. when the interval has been lengthened to 7 days. Evidently, there is a very gradual decrease in the reliability of recognitions placed in the category of "absolute certainty" as the interval between exposure and identification is increased. But it is well to emphasize that this decrease, as has already been pointed out, is due not to an actual increase in incorrect recognitions as the intervals are lengthened but to a decrease in the number of correct recognitions. Either tendency would produce the same effect of a gradual drop in the validity of the recognitions, but they give totally different interpretations to the situation. As it actually is here, *the drop in the validity is due to the fading out of impressions received at the exposure, not to the rise of new and false impressions.*

The validity of the recognitions in Pile No. 2 remains

practically constant, approximating 67 per cent. The third pile shows a validity of about 50 per cent. As was pointed out in the study of the effect of length of series upon recognition memory the validity of the third pile is no better than a random guess. For a random selection of 20 from 40 would gives us 50 per cent. of validity. This is likewise true here—the validity of the third pile is no better than what chance would warrant. It is interesting to note here in addition, however, that the validity of the third pile is actually higher than that of the fourth pile. Recognitions representing pure guesses are then lower actually in validity than are those when we feel there is a chance of their being correct. (The reason that there are consistently lower per cents. in Pile No. 4 than 50 per cent. is due to the fact that after all the recognitions in which the subject has any confidence have been made we have more incorrect than correct words to choose from in making a selection. The per cents. in Pile No. 4 are probably pretty close[1] to what chance would warrant, and these per cents. for the different intervals probably indicate the relationship between correct and incorrect words that are left to be chosen from after Piles No. 1, No. 2, and No. 3 have been determined.

4. *Results Combining (1) the Number of Correct Recognitions and (2) the Validity of the Recognitions.*—In order now to measure the total amount of correct recognition possible for the different intervals under study it is necessary to combine the results from the first three piles. This is done, as has already been pointed out, by adding to the score in the first pile, ¾ of that in the second and ¼ in the third pile. As the fourth pile only meant a guess it manifestedly has no value whatever in this connection. But it is not enough to simply add together the scores obtained from the correct recognitions. The number

[1] G. S. Fullerton and J. McK. Cattell ('On the Perception of Small Differences,' p. 151, 1892) have shown that "the observer is more apt to be right than wrong even when he feels little or no confidence in his decision." This point cannot be determined in this experiment due to the complexity of the situation. To the extent that it holds true here it would modify the statement in the text. However, in experiments where the stimuli to be recognized in the test are presented in order and a judgment as to one's confidence in the recognition is registered before proceeding to the next stimulus, the chance remains throughout 1 to 1 that one is correct. And even in such cases, the recognitions assigned to the doubtful class (Pile No. 3) are no better than chance would warrant for the great majority of subjects.

of incorrect recognitions must also be taken into account. Unless this is done an interval which is credited with 8 correct and 3 incorrect recognitions would be scored higher than an interval with 7 correct and 0 incorrect recognitions, which would be, of course, wrong.

Now perfect memory would in this case consist of selecting the 20 correct words and placing them in Pile No. 1. Zero memory would be any score equal to pure chance as 6 correct and 6 incorrect or 2 correct and 2 incorrect. The following formula[1] gives 100 per cent. for perfect memory, 0 per cent. for zero memory and an orderly progression of steps between the two extremes.[2]

$$\frac{\text{correct recognitions}}{\text{total number presented}} \times \frac{\text{correct} - \text{incorrect recognitions}}{\text{correct} + \text{incorrect recognitions}} \times 100.$$

For example, consider the score given in Table I.

Pile No. 1, 5 correct and 0 incorrect, ... $\frac{5}{20} \times \frac{5-0}{5+0} \times \frac{100}{1} = 25$ per cent.

Pile No. 2, 3 correct and 0 incorrect ..... $\frac{3}{20} \times \frac{3-0}{3+0} \times \frac{100}{1} \times \frac{3}{4} = 11.3$ per cent.

Pile No. 3, 3 correct and 1 incorrect, ... $\frac{3}{20} \times \frac{3-1}{3+1} \times \frac{100}{1} \times \frac{1}{4} = 1.9$ per cent.

Total ..... 38.2 per cent.

The calculations involved in the above example seem very complicated but after a little use the totals are readily obtained, since the several steps above can be reduced to just one in most cases.[3]

[1] For further information regarding this formula, see Strong, *op. cit.*

[2] I wish to correct here a statement in my previous article. It was there stated that the steps were *equal*. They are not, and that was known at the time. For example, the score for 10 correct and 1 incorrect out of a possible 20 is 40.9 per cent. while for 11 correct and 1 incorrect it is 45.8 per cent.—a difference of 4.9 per cent. The score for 10 correct and 9 incorrect is 2.6 per cent. while for 11 correct and 9 incorrect it is 5.5 per cent.—a difference of 2.9 per cent. An extra correct recognition should count more according as it is more unlikely to happen according to chance and this is just what our formula does.

[3] Any formula used in this connection, that I have been able to devise, has some objection to it, although several that I have used give within a few per cent. of the same final scores. The present formula penalizes mistakes a little more than is warranted on a basis of chance. If on the other hand, we subtract the incorrect from the correct

Using this formula a score was determined for each record obtained. A summary of these records is given in Table V. We have there summaries of the 5 subjects and then a final summary of the 15 records. Under each one of these six headings are given the average scores for the first three piles and then a summary of the three. (The details from which the averages are obtained for subject *A* appear in Table VI.) Many records appear with a minus sign attached. The negative score is due to the fact that more incorrect than correct recognitions were made in those records. Now practically there can not be such a thing as negative memory but statistically there can be and should be here. In order to offset the correct recognitions which were made by chance it is necessary to subtract the score where incorrect recognitions are made equally by chance,—hence the negative scores. And also, we must have some way of penalizing the subject who places, for example, 19 correct words in Pile No. 1 and 1 incorrect word in Pile No. 2. Such a score is not so good as if the 1 incorrect word had been placed in Pile No. 4. The two scores actually, in these cases, would be 91.3 per cent. and 95 per cent., respectively.

The probable errors given here are probable errors of the averages. They indicate a lower reliability to the figures than actually exists. This is true because of the presence of a very noticeable practice effect. If the different scores for each record were stated in ratios of the score for recognition "immediately after exposure" the probable errors from these ratios would be very much smaller. It is because of this that probable errors have not been given to the other tables. However, it might be added here, that they are approximately the same in Tables II., III., and IV. as shown here.

recognitions (as was done by B. R. Simpson in his 'Correlations of Mental Abilities,' Col. Contributions to Education, No. 53) and score on the basis of such differences we have a formula which does not penalize mistakes enough. For example, 10 correct and 0 incorrect in this experiment will be scored 50 per cent. by either of these two methods. But 12 correct and 2 incorrect will be scored 35.7 per cent. and 50 per cent. by the two methods respectively. The former possibly, as has been said, penalizes the mistakes too heavily, the latter does not penalize them enough. For 10 correct and 0 incorrect is certainly preferable in any sense to 12 correct and 2 incorrect.

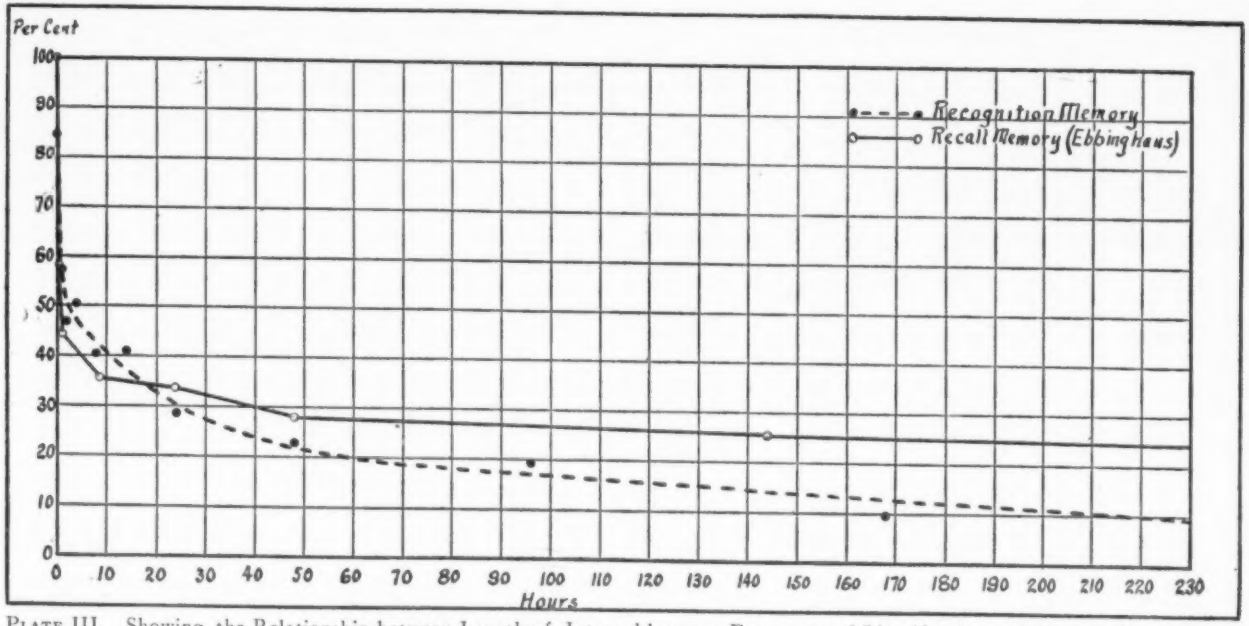

PLATE III. Showing the Relationship between Length of Interval between Exposure and Identification and Recognition Memory (when amount and accuracy of recognition are both considered).

Plate III. presents the totals for the 15 records in the shape of a curve. The dotted line is a "smoothed" curve from the results. The other curve represents the results secured by Ebbinghaus[1] in his study of recall memory. Ebbinghaus' first record was after 20 minutes—hence the very low first record on his curve (58.2 per cent.). Our 15 minute record was 62.7 per cent. and our 30 minute record was 55.5 per cent. Evidently there is no difference between his initial record and what would have been found here in this study of recognition memory for an interval of 20 minutes. The two curves are very similar in shape and almost agree as to actual amounts until after the 4 day interval. It seems possible that from there on Ebbinghaus' curve represents a greater amount retained than does our curve. It is certainly higher than ours at this point as well as at 31 days. It is also considerably higher than the individual record of *A*—our best subject. But still two records of *A* and one of *B* are higher than Ebbinghaus' average and two more records, one of *A* and one of *C*, approximate it pretty closely. After all, then, it may represent nothing more than an extreme individual difference.

In Plate IV. are presented similar curves to those in Plate III. but for the two subjects *A* and *B* separately and the three other subjects, *C*, *D*, and *E*, together. Each of the three curves is based on an average of 5 records for each interval studied, except the 42 day interval. The relative relationship between the three curves remains constant, barring irregularities due to the few cases. Subject *A* is evidently superior and *B* inferior to the group of the three subjects.

We should conclude that *there is no difference in the form of the curves for retention in recall and recognition memory.* The amount retained at any particular point in such curves depends, of course, upon the material used and the method employed.

One more point in Table V. might be mentioned. The individual records as well as the summary show a steady decrease in the score in Pile No. 1 as the interval studied is lengthened. The score in Pile No. 2, on the other hand, increases slightly but very irregularly. The score in Pile No. 3 approximates

[1] Ebbinghaus, *op. cit.*, p. 103.

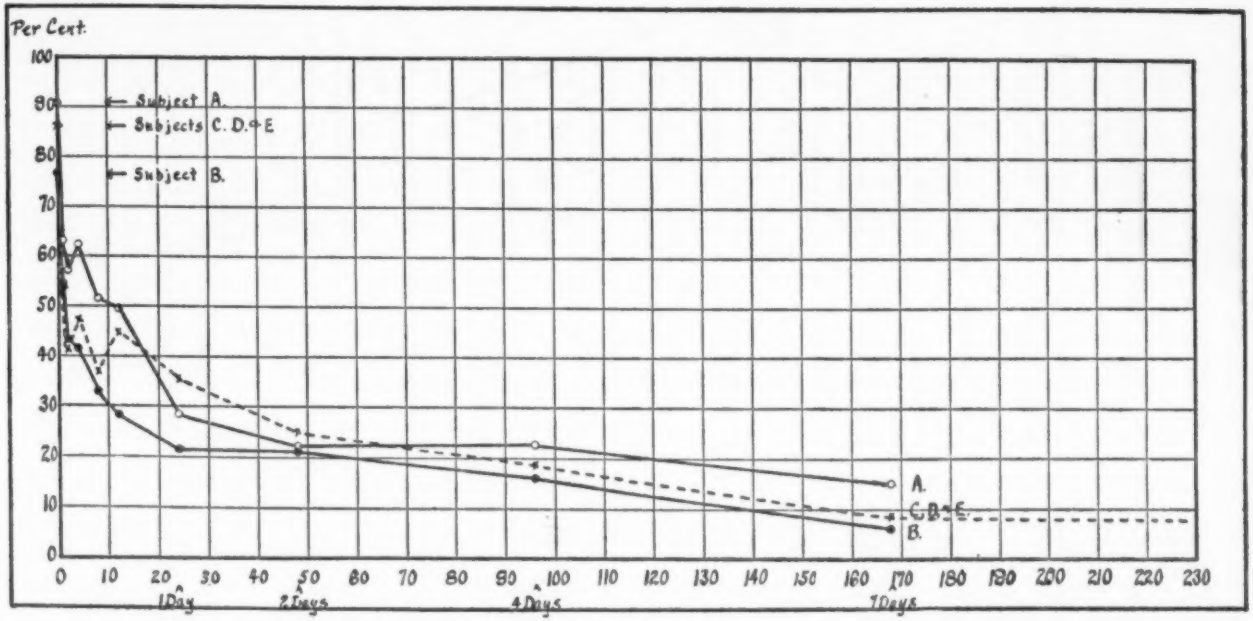

PLATE IV. Showing the Records of Subjects A and B separately and C, D, and E together.

zero. This is the same situation as was found in studying the effect of length of series upon recognition memory. In both cases the score in Pile No. 1 decreased as the difficulty of the task was increased, the score in Pile No. 2 remained practically constant, and that in Pile No. 3 was practically zero. As stated there, "*evidently then recognitions that are not accompanied with a feeling of "absolute certainty" are practically no better than random guesses.*" In this study also they are no better in Pile No. 3, and in Pile No. 2, though they do amount to something, it is very little when compared with Pile No. 1.

5. *The Effect of Practice on Recognition Memory.*—Table VI. presents a complete detailed record of all the experiments on subject *A*. The records for each pile (or degree of certainty) are given for each separate experiment and then the score that is obtained from each of the experiments is shown in the fifth column. Under each pile appear two columns of figures—the first column refers to the total number of correct recognitions and the second column refers to the total number of incorrect recognitions which were assigned to that pile in each separate experiment. For example, opposite the 15 second interval we have the numbers respectively, 15—0, 0—1, 3—1, blank, 73.2. These figures indicate that in the first experiment on this interval 15 correct and 0 incorrect recognitions were assigned to the first pile (absolute certainty), 0 correct and 1 incorrect were assigned to the second pile (reasonable certainty), 3 correct and 1 incorrect to the third pile (doubtful certainty), and no recognitions were assigned to the fourth pile (not then being used). The score for these results is 73.2 per cent. of a perfect record. Below the averages for subject *A* are given the corresponding averages of subjects *B*, *C*, *D*, and *E*. (It must be remembered in this connection that Pile No. 4 was not introduced with subjects *A* and *B* until they were half way through the third record.)

A study of the average scores in the bottom rows of this table gives us some idea of the way improvement took place with these subjects. These figures are plotted in Plate V. The records of *A* and *B* show a very noticeable improvement,—an improvement from 42 per cent. to 63 per cent. with the

first and an improvement from 27 per cent. to 53 per cent. with the second. Such improvement in memory should be explained if possible.

Throughout this experiment it has been considered that we were studying a memory function. And in the generally accepted use of the term that is correct. But it must be remembered that in this experiment we have not separated the pure memory function, whatever that is, from many other functions which have entered into the total process. And now in

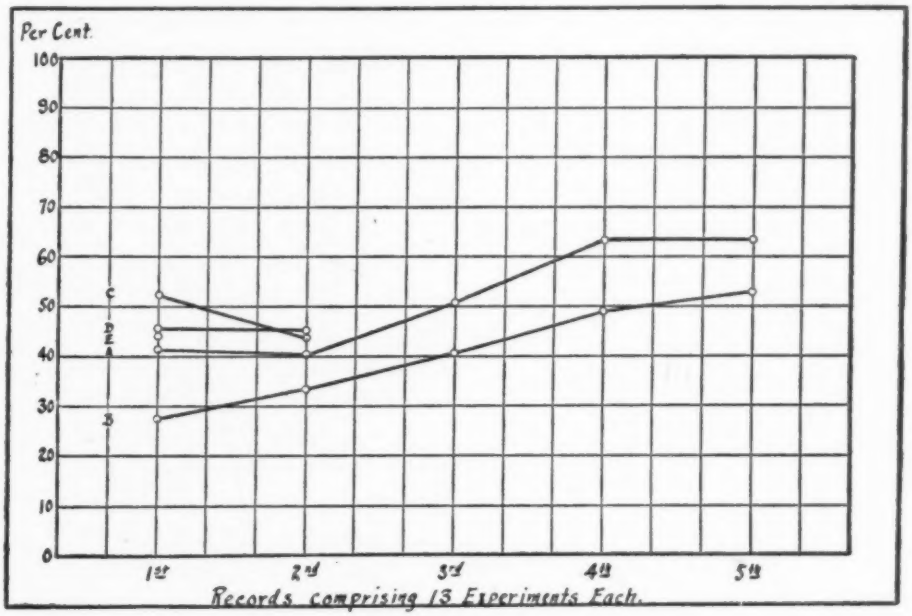

PLATE V. Showing Practice Effect. Each record represents the average of the scores for the thirteen different intervals studied.

attempting any explanation of the improvement which *A* and *B* have shown we must take into consideration all of the possible processes which have influenced the work. Roughly speaking, we have the three divisions,—the sensory reception of the stimuli, their retention, and the expression of them at the appropriate time. The question is, was the total improvement due to an improvement in one of these three general divisions, or to two of them, or to all three of them?

Let us study the details of the improvement first and then return to our general question.

From the data from subject *A* it is evident that there was a slight drop in the second record as compared with the first. Introspection furnishes a possible explanation of this. The first few experiments were new and interesting. The subject found no difficulty in applying herself to the exposure lists. But after a short time the experiment became a matter of course, and the enthusiasm died out. That is, spontaneous attention failed or at most came only in spurts. The first record is to be considered somewhat in the light, then, of an initial spurt—a time when the newness of the experiment kept one interested in it. The second record of *C* likewise shows a drop. The experimenting in her case with the second record dragged on for nearly two months. The experimenter was very much bored with the experiment at that time and although introspections of *C* throw no light on the subject, she at least took very little interest in the work at that time. The results of *D*'s two records show no gain or loss. The average scores of *B*, on the other hand, show no drop in efficiency but instead a steady rise. Introspections by him show that at no time did he lose, at least consciously, any of the freshness with which he first attacked the work. It seems then very reasonable to suppose that the drop in efficiency in the second record with subject *A*, and probably with *C*, and the no-gain with *D* are due partly at least to a loss of spontaneous attention. If this was the only factor operating we should expect a steady drop in efficiency from record to record. But there were other factors.

First, subject *A* reports that she consciously attempted to improve the accuracy with which she assigned recognitions to Pile No. 2. Her first introspection showing such a need occurred when she was half way through the first record. As is shown later, many words seem to the subjects to have been in the exposure list when they were not there at all. They wavered between placing them in the first or second pile. But it was soon realized that the recognition of these words had a different feeling-tone from many of the other recognitions.

When this was learned the subject ceased to consider them for either Pile No. 1 or No. 2. This tendency is reflected in the averages under Pile No. 2 in the table. These averages show a decided drop in the use of Pile No. 2 during the second record as compared with the first record for subjects *A* and *B*. From there on there is a steady increase in the number of correct recognitions coupled with about a constant number of incorrect recognitions with *A*, while with *B* there is a similar tendency to improve the proportion of correct to incorrect recognitions but here shown by a decrease in the number of incorrect recognitions. After the first tendency not to use the second pile there came the more intelligent use of it. Subjects *C* and *D*, who could not scrutinize another's records, as could *A* and *B*, did not learn about the tendency to make mistakes in Pile No. 2, as the records show.

Another factor which operated to raise the scores was the more careful use of Pile No. 1. The averages for both subjects *A* and *B* at the bottom of Table VI. show a very decided improvement in this respect. *B* learned quicker than *A* here for he has practically no mistakes in Pile No. 1 in his second record, while it was not until the third record that *A* attained this same degree of perfection. Yet *A* was conscious of the need for such improvement by the time she was half way through the first record according to the date of her first introspection on this subject.

It is quite evident, then, that both subjects *A* and *B* learned to distinguish better between correct recognitions and incorrect ones. (Some of the points brought forth from introspections on this point will be given later.) But not only improvement in the quality of the work took place but also a decided improvement in the quantity. Consider the scores from the third and fourth records for both subjects *A* and *B*. Here there was no improvement in decreasing mistaken recognitions in Piles No. 1 and No. 2. But there was a decided gain in the number of correct recognitions assigned to the two piles by both the subjects. How shall we explain this improvement? Was it an improvement in the processes of 'taking-in' the impressions or in the processes of retention? The data at our disposal do

not permit us to answer; they throw no light on this point beyond merely recording the fact that there was such improvement. Introspection throws some light on the subject, however. Over and over again after the first few experiments the introspections record the fact that both *A* and *B* were consciously forcing themselves to 'grasp' each word in the exposure list,—not to be content with merely automatically looking at the word and pronouncing it but to actually comprehend what it meant.

This process of comprehending seems analogous to the flow of electric current. If the current does not flow somewhere there is no current in the wire. In the same way it seemed that if the stimulus word did not cause a thought current to flow somewhere there was really no current at all—there was no comprehension of the word. Comprehension of nouns with subject *B* was very often accompanied by localizing the object which the word stood for. This localizing was accompanied many times by a nod of the head in the direction of the place. Whether he always nodded his head or not *B* does not know but whenever he watched himself carefully he found himself doing it. For example, to the word 'kettle' there was not an association with 'kitchen' in the sense of definitely thinking 'kitchen' nor an incipient pronunciation of the word, as far as *B* could determine, but a general feeling of 'kettle in kitchen' and a nodding of the head in the direction of the kitchen. Later, on recognizing 'kettle' 'kitchen' came to mind. But the characteristic thing about the association was the emphasis upon the direction of the object rather than the object itself. Adjectives were similarly very often localized by assigning them to appropriate objects whose location was known. Verbs and adverbs were very often applied to the subject, himself, as though he was going through the action. For subject *B*, then, comprehension involved in a great many cases the holding of the word long enough in the mind for it to result in an incipient movement of the subject's body. This was not an easy task for many words. Such words did not readily provoke a response and it required voluntary attention to hold them in the mind until they would do so.

From the introspections of *B* it would seem that the improvement shown in this study in recognizing more and more words correctly was due to conscious effort in the reception of them.

The process of comprehension was slightly different in the case of *A*. She was not conscious of any tendency to localize but in attempting to realize the full meaning of each word she inevitably formed certain associations with it. These associations were all of the simple and obvious type as they were entirely spontaneous. They increased in number with practice. In the early part of the experiment there would be words in each list with which *A* formed superficial associations as of sound or of similarity of appearance of the word. By the time of the fifth record however, practically every word was comprehended clearly and all associations were obvious. "For instance, whereas, 'kettle,' if it had been used in the first record, might have called up 'kittle cattle,' by the time of the fifth, it would certainly have been associated with 'kitchen' or 'cooking'" (introspection of *A*). Reference to the next section will indicate how this change materially aided in the identification of the previously shown words.

Returning to our question, How shall we explain the improvement? We must answer it by the simple statement that introspection clearly indicates a change in the method of receiving the original impressions while the improvement was taking place. That the two are related seems most probable. That *A* and *B* were able to improve the accuracy of their recognitions through watching the mistakes of their subjects and that the means of accomplishing this improvement depended upon a better analysis of the feelings accompanying the recognitions seems to imply that there was a change in the control of the response to the experiment. Whereas, at first recognitions were made with little or no guidance, they later were made under careful scrutiny and according to standards justified by experience. We should conclude, then, that *the improvement in quantity is due very largely to improvement in methods of 'taking in' the exposure words, and that improvement in the quality of the work is due very largely to the better control of the response to the tests.* But as to whether there was any change in the process of retention, we cannot say.

6. *Some Information Concerning the Process of Recognizing Words as Revealed by Introspection.*[1]—A word, first of all, should be said as to the general reaction to the lists. Once the list had been read and the problem in mental arithmetic solved it seemed to both *A* and *B* that the list was gone forever. There seemed in many cases that there was no string (figuratively speaking) which by pulling could bring back the list nor any of its contents. The words were gone. For example, one remark of *A* immediately after finishing the problem was, "I haven't a single word carried away." This was not strictly true of all of the lists. In many cases the subjects felt that they might recall the words if they would, especially did they feel this way immediately after reading the list. But after the list had been retained some time, the feeling would come that the whole thing was gone. Not only was this so but it was found very difficult to keep track whether one was in the midst of an experiment or not. A rather favorite expression of both *A* and *B* was, "Have I a list down me?" and when told they had to reply, "I have? I don't remember it." Similar remarks were noted from subjects *C* and *D*. This 'lostness' of feeling was quite characteristic.

No attempt was made to study those feelings which generally give one an idea as to whether they have done well or not. But on two very definite occasions when *A* reported that she knew she had not done so well as usual her work was fully up to average. On one of these occasions she had a headache and on the other she had just been wakened from a nap and did the work although insisting that she was still too sleepy to do it right. On another occasion *B* was sure he had done poorly and actually had not. Moreover, he felt no different from his usual state, on the two occasions when he made proportionately the two poorest records.

In order to give a better idea of the information obtained from the introspections a characteristic record is given here. It is the first test on *D* for 1 hour. Those words of the 40 in the second list are given here which were in the exposure list

[1] This section is added as simply a statement of what was found in this study, not as an attempt to prove or disprove current theories as to recognition memory. The writer hopes, however, to take this subject up at some future time.

together with those words which were wrongly identified. The others have been omitted. The first column of figures gives the position of the 20 correct words as they appeared in the exposure list. The second column gives the degree of certainty which *D* attached to his recognitions. Then follow the words and the introspections.

11	1	champagne—	mistook it for campaign (T. R. and Taft) which I remembered doing before.
20	1	rogue—	at end of list, noticed it particularly and chuckled over it, feeling remembered.
19	1	firm—	either an adjective or firm (company), thought of that when first reading list through.
18	1	crocodile—	visual image of leather on grip.
13	2	haunted—	quite sure I had that word, don't often think of haunted things but I know I have had a thought of such recently.
16	—	song	
—	3	divorce—	mere guess.
17	1	penny—	association of woman who dropped a cent and made some ado picking it up.
—	2	marvel—	have thought of 'marvel' recently, can't think of any possible connection except the list.
10	—	breath	
6	1	fable—	association of book just read in which a fellow makes a pedantic talk about telling fables to children to teach them.
—	2	echo—	like haunted above.
—	4	puddle	
3	4	handerkerchief	
5	1	corner—	read it first as 'comer.'
—	3	den—	have thought of 'den' recently, can't think of any possible connection except the list.
7	3	continent—	had this word in a previous list, quite sure it was not in last list.
1	—	ugly	
2	—	pass	
—	3	cab—	mere guess
4	—	bill	
12	1	insect—	not quite sure it wasn't 'insert,' that seemed to vivify the word.
9	—	cheat	
15	1	pressure—	associated with tank. Did it now and then remembered doing it before.
14	1	fifty—	associated it with sixty in previous list.
8	1	period—	know it was in list.

Summary.—Pile No. 1, 11 correct and 0 incorrect, score 55.00 per cent.
Pile No. 2, 1 correct and 2 incorrect, score −2.50
Pile No. 3, 1 correct and 3 incorrect, score −1.90
Pile No. 4, 1 correct and 1 incorrect, ——
Total..... 50.60 per cent.

A possible objection to the validity of these introspections might be made on the ground that they were merely retrospections,—that they were merely reasons trumped up afterwards to explain the recognitions. Very likely some of them are no more than such 'after-thoughts.' But in the great majority of cases the subjects were just as sure that these various associations, that are given in the introspections, were actually present at the time the words were first read in the exposure lists as they were sure of the recognitions of the words themselves; in fact, even more so. The introspections had the same relation to the words as the occasion of meeting someone has to the recognition of that one again. The recognition is confirmed as the occasion comes to mind.

From such records as that given above the following points have been noted concerning recognitions of which the subject is absolutely certain. Recognitions are made:

1. When the word was recognized by itself without any other association of any sort coming to mind. For example, 'tarry—I know it was there' (*A*), or 'period—know it was in the list' (*D*), 'pride—don't know why' (*C*). Such introspections were comparatively rare for words assigned to Pile No. 1. Recognitions with such introspections were at first most frequently assigned to Pile No. 2 and later to Pile No. 3. Many words with such introspections were wrong, they had not been seen in the exposure list. It was the questioning of these recognitions that contributed to raising the accuracy of Pile No. 2 for *A* and *B*, referred to above.

2. Recognitions are made when at sight of the word in the second list an association with it came into consciousness. The association was then recognized as having been met with before—very often directly connected with the exposure list —and then the word was identified as having been in the exposure list. Generally speaking, as soon as the association came to mind the subject was sure of the recognition. Some recognitions, however, appeared actually to come as the result of a train of thought as outlined above. Whether this train of thought was necessary or not cannot be determined. But it does seem certain in many cases that in some way recogni-

tion of the brute word would not take place until after the previously formed association with it came into mind. The associations were of great variety. Several distinct types may be mentioned. (*a*) An association between the word and some idea or object. (*b*) An association between the word and some feeling or emotion. For example, 'waft—wäft or wăft, remembered the quandry, not the word' (*D*); 'rid—last word in the list, good riddance' (*E*); 'insect—have such a horror of them, would remember, (*E*). (*c*) An association between the word and a visual impression. For example, misspelled words, words with 'q's' in them, as quarry, or double 'oo's,' as in footstool, or again as 'button—at first not recognized, later remembered difficulty of reading handwriting of this word the first time and then identified it' (*B*). (*d*) An association between the word and a motor response. For example, 'whelp—hard to pronounce, and then recognized' (*A*). (*e*) An association between the word and an auditory impression. For example, 'throb—sound, I think' (*A*), 'hustle—the sound of it' (*A*), 'ant—because I corrected myself to say aunt before' (*D*).

3. Recognitions were made on the basis seemingly of associations that did not themselves come to consciousness, at least until after the recognition had been made, but which were felt to be there when the recognition was made. For example, "gastric—made some unpleasant association before—the unpleasant feeling comes up now but I can't recall the association. I know I saw the word because I recognize the feeling" (*A*); 'night—forgotten association—think it was with nightingale' (*A*); 'moon—didn't recall previous visual image until after word had been checked' (*A*); 'primitive—some association connected with this, can't recall.' Same with 'chance.' Later remembered it was 'primitive queen by chance,' thus connecting three words in the list together (*B*); 'trinket—feeling as though I had an association with it' (*B*); 'moisture—faint idea of association with rain' (*B*); etc.

Recognitions with the accompanying feeling of absolute certainty seem to depend upon the fact that the same mental process has accompanied the word the second time that did

the first time. In other words, when a new association came up on the second presentation the word was not recognized. In support of this point we have many introspections made after the checking of the list was finished and the subject was told the mistakes he had made. Then the first associations with many of the words would come to mind and the subject would realize that if it had come at the time of checking the list the word would have been recognized. It is right along this line where *A* made one of her conscious attempts to improve. She realized that when trifling associations, as of sound, came on reading the exposure list, that very often they did not appear the second time and so the word was not recognized. It was in this way that she learned to inhibit such associations or in other words not to be content in reading the list with "grasping" simply visual or sound peculiarities of the word.

It is possible that the recognition takes place as soon as the word is seen but all our introspections suggest that the recognition is more likely made while the accompanying associations are coming to consciousness the second time. Whether this is due to the mere fact that a larger content—the word + its associations—is more easily identified than a smaller one—the mere word—or whether recognitions take place when only the association centers are aroused is an interesting question to speculate over.

Let us now note a few differences in attitude that appeared which directed the use of the various grades of certainty. *D* said at one time: "With Pile No. 1 I let myself go just as my feelings direct; with Pile No. 2 I hunt up some reason for my choice and when I get it I feel justified in checking the word; and with Pile No. 3 I again let myself go as my feelings direct." None of the other subjects gave such a summary of their attitude toward the three piles. But *A*, *B*, and *E* all made such remarks as this one of *D* on referring to several words he had placed in Pile No. 2: "I think I saw those words in the list, but I have no way of justifying it. I shouldn't be surprised to find that they were less correct than those placed in Pile No. 3." As already pointed out *A* and *B* early realized

that they were making many mistakes in the recognitions assigned to Pile No. 2 and deliberately attempted to rectify the tendency. *E* also expressed herself quite emphatically that she had very little confidence in those recognitions she assigned to Pile No. 2. Her record (see Table VI.) shows that she made as many incorrect as correct recognitions. The situation with regard to this second pile was very curious. While checking the list Pile No. 2 meant a degree of certainty between No. 1 and No. 3 but afterwards on thinking it over, one felt that Pile No. 2 was less reliable than No. 3. The reason for this was in the case of *D*, as already pointed out, that guidance toward Piles No. 1 and No. 3 was on the basis of feelings of acquaintance with the word, while toward No. 2 it was on some other basis. *B* felt in somewhat the same way that there was a difference in quality between the recognitions in Pile No. 2 and the other two piles but he could not express what the difference was.

Pile No. 3 was made up largely of words which were recognized on little or no more basis than what is meant by the word 'guessing.' On many occasions the word seemed familiar and no other cause for its familiarity could be given than that it was in the previous exposure list and so it was checked. Another example of the feeling basis for these recognitions is illustrated by this introspection of *A*,—'marshall—no associations, nor reasons for checking come to mind. I just feel that it was there more than the other words. More a negative feeling toward the others than a positive feeling toward this word.'

Conclusion

1. When recognition is allowed immediately after presenting a list of 20 words 84 per cent. of them will be identified correctly and with a feeling of absolute certainty, while only 10 per cent. can be so recognized after an interval of 7 days.

2. The per cent. of such correct recognitions decreases very rapidly at first and then more and more gradually as the interval between exposure and identification is lengthened.

3. As the interval is increased between exposure and recognition the certainty with which the recognition is made steadily decreases.

4. Actually, very few incorrect recognitions are made as compared with the total number of correct recognitions. Nor does the per cent. of incorrect recognitions materially increase as the interval between exposure and recognition is lengthened. "It seems then that the ability to know we have not seen an object is much more firmly fixed than the ability to pick out what we have seen."

5. When the two factors of the per cent. of correct recognitions from the total number possible and their validity (comparing correct with incorrect recognitions) are taken into account, we obtain a relationship between recognition memory and the length of time between exposure and identification practically identical to the relationship found by Ebbinghaus in his study of recall memory.

6. Recognitions not accompanied with a feeling of 'absolute certainty' are little better than random guesses.

7. A very noticeable improvement in recognizing words previously exposed is shown. This improvement is shown in two ways: (1) by a decrease in the number of incorrect recognitions made, and (2) by an increase in the total number identified correctly. The former is due primarily to a better analysis on the part of the subject of the feelings accompanying recognition. The latter is due to an improvement in the methods of perception of the stimuli as they are exposed.

8. Recognition seems to depend upon the fact that the same mental process has accompanied the word the second time that did the first time.

9. Recognition ordinarily seems to take place as the associations, which were formed when the word was first seen, come up again into consciousness.

10. Increasing the difficulty of recognition by increasing the length of series exposed results in a slow but almost constant decrease in the ability to recognize correctly. But increasing the difficulty of the task by lengthening the interval between exposure and recognition results in a very rapid decrease in the ability to recognize up to about an interval of two hours and from there on a less and less rapid decrease in this ability.

A NOTE ON THE ROTARY CAMPIMETER

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In reviewing the reports of apparatus published in 1912, Professor Seashore says:[1] "Ferree states that the object of this apparatus is to add to the vertical campimeter the rotary features of the perimeter, and thus to allow the investigation of every possible meridian with as much ease and precision as was possible with the old form of campimeter in the nasal meridian only or at most in the nasal and temporal meridians. For a full description, one must consult the original. To the reviewer it seems unfortunate that the apparatus was not built for operation on the surface of a hemisphere rather than on a plane surface." In reply to this criticism by Professor Seashore the writer[2] wishes in the first place to point out that in choosing a plane surface he has introduced no exception. He has rather followed a rule that has been uniformly adopted in the construction of campimeters, beginning with the apparatus described by Hess in 1889.[3] If, then, Professor Seashore's criticism has any value at all it applies just as strongly to the older forms of campimeter as to the one criticized. In the writer's opinion, however, this criticism has no practical value

[1] C. E. Seashore, 'Apparatus,' PSYCHOLOGICAL BULLETIN, 1913, X., p. 33.

[2] The writer has felt urged to reply to this criticism by Professor Seashore by the following reasons. (1) The criticism is so natural to one who has been thinking along certain lines that it was the writer's intention to anticipate it in the original description of the apparatus. Lack of journal space, however, rendered it at the time inadvisable. (2) As stated in other places by the writer, all investigations of the comparative sensitivity of the retina to the different colors should be made in daylight illumination, and conclusions may be drawn only when a broad canvass of sensitivity in many meridians has been made. Since both of these objects can be accomplished so conveniently and so well by means of the rotary campimeter in the form described by the writer, it seemed to him that the extension of its usefulness should not be deterred for want of the answer which can be so readily given to Professor Seashore's objection.

[3] C. Hess, 'Ueber den Farbensinn bei indirectem Sehen,' *Archiv für Ophthal.*, 1889, XXXV., 4, p. 25.

whatever. But it is a criticism that is very likely to be made at first thought by one who has had little experience with campimeters. For example, earlier in his own experience the writer too shared Professor Seashore's opinion as to the relative merits of plane and hemispherical surfaces for the campimeter screen. In fact his first model of the rotary campimeter was constructed with an hemispherical screen. His first attempt to use this hemispherical screen, however, led him to discard the principle for all time as applied to an apparatus designed for daylight work.

A statement of the apparent advantages of the hemispherical screen and its prohibitive disadvantages is probably the best way in which to show why the writer has adopted a plane screen for his campimeter. The apparent advantages that have been conceived by the writer or suggested to him are as follows. (1) In conversation with the writer Professor Seashore contended that in case of a plane surface there would be different amounts of light reflected to the eye from the colored surface at different angles of excentricity, and that the difficulty, he thought, would be obviated by the use of a hemispherical screen. This criticism would have validity providing that the light falling on the stimulus comes from a single source and that the surface of the stimulus gave specular and not diffuse reflection. Neither of these conditions obtain, however, when the campimeter is properly used. When properly used, the room in which the work is done is uniformly illuminated by daylight, hence the light comes from no particular direction. Moreover, the pigment surface from which the light is reflected to give color does not give specular reflection. The reflection is diffuse. Furthermore, so far as reflection is concerned, the writer cannot see that the situation would be materially different if a hemispherical surface were used. In either case the stimulus would be exposed at different angles of excentricity with the line of sight and if different amounts of light were reflected to the eye in the one case, so would there be in the other. There would be a difference in the two cases only should the eye be kept stationary and the stimulus be moved to give the different exposures in the peripheral field of

vision. In that case the surface of the stimulus would always be normal with the lines connecting it with the eye in case of a hemispherical, while it would be inclined to this line in case a plane surface were used. But even here no advantage is gained for the hemispherical surface for the light would be incident upon the surface of the stimulus at different angles for different degrees of excentricity unless the source of light could also be in the line connecting the stimulus with the eye which is, of course, entirely out of the question. Moreover, in an apparatus designed so that the stimulus is kept stationary at the center of the screen and the exposures are given at different degrees of excentricity by taking successively different points of fixation towards the periphery of the screen, as is the case in the apparatus described by the writer, the surface of the stimulus is always normal to the line connecting it with the eye. Therefore, so far as the question of amount of light reflected to the eye at different degrees of excentricity is concerned, we have exactly the same situation with this type of apparatus when the surface of the screen is hemispherical as when it is plane. (2) The writer has himself two objections to offer which seem to him more plausible than are those offered by Professor Seashore. (*a*) When the screen is a plane surface, the visual angle subtended by the stimulus is decreased in the meridian in which the sensitivity of the retina is being investigated, and in different amounts for different degrees of excentricity. This is a real difficulty and there is no way to obviate it unless there be a compensating enlargement of the stimulus opening in the same meridian. This is not by any means an impossible remedy, especially if the stimulus-opening be made in the shape of a quadrilateral, adjustable from either of the four sides. In fact, in the new model of the rotary campimeter now being constructed in this laboratory, this remedy is being applied. (*b*) If the stimulus-opening is at the center of the campimeter screen and the fixation points are laid out on the surface of the screen, the stimulus and fixation points are all at different distances from the eye in case a plane surface is used for the campimeter screen. This, it would seem, might lead to a disturbance in the accommodation of the lens for the

stimulus. Whether or not this disturbance, if it actually exists, has any effect on the limens or limits of sensitivity in the different meridians was carefully tested out by the writer before adopting the plane surface for his screen. For example, in one series of experiments the limens and limits were determined when the fixation points were carried on an imaginary arc with a radius equal to the distance of the stimulus-opening from the eye, and in another these determinations were made with the fixation points laid out on the plane surface of the screen. In no case was a difference in result obtained for corresponding points in the two series of determinations greater than the normal M.V. for either series. The conclusions can be drawn, therefore, that even should disturbances of accommodation be produced, they are not of sufficient magnitude to affect the determination of color sensitivity.

The prohibitive disadvantage of the use of a hemispherical surface for the screen is that it is quite impossible to get a uniform illumination of such a screen when the work is done under daylight illumination. Nor is it possible to control the degree of illumination of the screen. The primary object of the use of a campimeter is thus defeated, for the apparatus was devised in order that the field surrounding the colored stimulus might be uniformly illuminated, and be made of whatever degree of intensity that is desired. When a hemispherical surface is used the stimulus-opening and the region immediately surrounding it are in deep shadow while the edges of the screen are illuminated to a degree roughly corresponding to the general illumination of the room. Between the edges and the stimulus-opening intermediate degrees of illumination are obtained. Still worse, the observer's eye must be at the center of curvature of this hemisphere, *i. e.*, the observer's head must in part fill in the opening of the hemisphere. The shadow cast by the observer's head would of itself render the use of this type of apparatus absolutely out of the question. In short, as plausible as the use of a hemispherical screen may seem at first thought, the writer feels safe in saying that 5 minutes spent in the attempt to use it in connection with the type of apparatus described in the article reviewed by Professor Seashore would

be sufficient to convince any worker in this field of the impracticability of this kind of screen for daylight work.

There is a possibility that a hemispherical screen might be used for work in the dark-room. A translucent screen might then be employed, illuminated by transmitted light falling on the convex surface of the screen. Until an artificial light is obtained, however, which approximates daylight more closely than any that has yet been described, this type of apparatus may be considered as out of the question for work in color sensitivity.

EXPERIMENTAL CRITERIA FOR DIFFERENTIATING MEMORY AND IMAGINATION IN PROJECTED VISUAL IMAGES[1]

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I. Introduction

The purpose of this paper is to discuss the differentiation of memory and imagination as indicated in a series of experiments on projected visual images. The series in question was not devised exclusively with this end in view, but also forms part of a larger investigation, as yet unpublished, on the psychology of meaning. Various series of experiments had been performed which aimed to secure introspective reports upon meaning-processes aroused by words, aphorisms, and simple problems of a mathematical and logical nature. The data from these experiments included sensations, images and feelings, but in addition there appeared factors which seemed to resist analysis into the categories named. It therefore occurred to me that a valuable sidelight regarding this important issue might be obtained from an additional set of experiments in which the observers should be instructed to obtain images in the three classes of sensation most important for representation, *viz.*, vision, audition and kinesthesis. Three series of experiments were accordingly devised and executed in which the observers were instructed to secure visual, auditory and kinesthetic images, respectively, appropriate words being used as stimuli for their arousal.

In the visual series, which is here the basis of report, the experimenter had definitely in view a study of the criteria by which memory and imagination may be differentiated, with special reference to the results then but recently reported by Mrs. Perky in her 'Experimental Study of Imagination.'[2]

[1] From the psychological laboratory of the University of Tennessee.

[2] *Amer. J. of Psychol.*, 1910, *21*, 422–452.

The series was begun about the middle of November, 1910, and completed in March of the following year, that is, before the appearance of the works of Koffka[1] and Miss Martin[2] which also bear upon this theme.[3]

II. Method of the Experiments and Preliminary Investigations

The experimental setting was the same that had been employed in the principal series for studying the meaning of words. The stimuli were exposed in a card-changer of the Alber type. The reactions were executed with a Römer sound-key, and registered by means of a Hipp chronoscope. The object of the experiments was to secure visual images suggested by the word-stimuli. In order to facilitate this, a circular disk was pasted upon the card, and the observers were instructed to fixate this when the card appeared. The complete instruction which was read to the observer at the beginning of each experimental period follows: "Hold yourself passive during the fore-period. When the card appears, fixate the disk on the right, and try to get an image suggested by the word which is printed below. As soon as the image becomes clear and definite, react with 'Peh!' Then introspect as completely as possible."

Seven observers were engaged in these experiments: Misses Miller (M.), Avery (Av.) and Kehr (K.), and Messrs. Adkins (Ad.), Philips (P.), Brewer (B.), and Davis (D.). The first two were teachers in the local school for the deaf and dumb. The last five were students in the University of Tennessee. Obs. Ad. was a graduate student; the others were undergraduates in their senior year. All had had instruction in psychology, and proved themselves very capable observers. Only three, Av., Ad., and D., had participated in the preceding work of the investigation on Meaning. I take this occasion

[1] 'Zur Analyse der Vorstellungen und ihrer Gesetze.' Leipzig: Quelle und Meyer, 1912, pp. x+392.

[2] 'Die Projecktionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder,' *Zsch. f. Psychol.*, 1912, *61*, 321–546.

[3] I regret that I have not had access to Milhaud's 'La projection externe des images visuelles,' *Rev. Philos.*, 1894, *38*, 210–222.

to express to them all my indebtedness for the hearty co-operation which they afforded me. Five of these observers experienced no difficulty in securing visual images which were usually clear. Two, however, (Ad. and D.) found great difficulty. In the case of one of these (D.) the positive evidence of any visual imagery is so slight that I have not included his results in this report. From Obs. Ad.'s results I have selected only those experiments in which some vestige of visual imagery appears evident.

A series of preliminary experiments was performed by all observers except M. They varied in number as follows: Av., 18; K., 10; Ad., 26; P., 27; B., 25; and D., 30. In these, different types of disk were tested, and the stimuli-words were at first called out by the experimenter as the card appeared. The procedure was then modified, the words being typewritten on slips which were pasted just below the disk in the center of the card. No special difference was noted in the character of the imagery when the stimulus-word was pronounced, as against its visual exposure. The latter method, being more conducive to an exact measurement of the reaction-time, was adopted for the main series. Disks were tried of black, white, different shades of gray, and a "granite" paper of bluish cast. No special influence upon the images being noted, the granite paper was selected for the main series, because of its greater suggestiveness in the cases of the two observers mentioned whose visual imagery was weak and uncertain.

The experiments of the regular series were devised with special reference to the results obtained by Perky. Fifty words made up the complete series. These were selected with the intention of suggesting memory and imagination in approximately equal proportion. The list follows:

Purse	Griffon
Diver	Bijou
Bed	Pfennig
Hamlet	Glasses
Pen	Flora
Castle	Mother
Taft	Gauntlet
Lyre	Belt

Viaduct	Malay
Endymion	Roosevelt
Watch	Hussar
Duke	Ring
Dean	Faust
Cerberus	Post-Office
Slippers	Centaur
Druid	Pencil
Market	Hun
Shilling	Sister
Father	Dragon
Khedive	Collar
Letter	Fairy
Gargoyle	Rink
Station	Pegasus
Apollo	Co-ed
Dr. Ayres	Plato

Two of the observers, M. and Av., performed but 25 experiments each. The others completed the full series.

It will be recalled that Perky secured some very striking results, which indicated an almost invariable accompaniment of eye-movements with memory-images, and a corresponding dearth of these with images of imagination. Her experiments were performed in a dark-room, with delicately arranged lights, which fell, under normal fixation, within the area of the blind-spot. Any appearance of these lights within the field of vision, accordingly, indicated eye-movement. It occurred to me that I might be able to test this result by the use of distraction-marks upon the exposure-card placed to one side of the disk. The aperture of the card-changer measured 6 by 2¾ cm. A disk 2 cm. in diameter was pasted upon the right side of the card within this space, allowing room below for the stimulus-word. This left a blank area, approximating 3½ cm. in width at the left of the disk. The observer sat with both eyes open, at a distance of about 30 cm. from eyes to card. The distraction-marks, made with a pen and black ink, were placed on the blank card near the left side of the aperture. Since these marks contrasted more strongly with the white background than did either the disk or the stimulus-word (printed from a violet ribbon), and were clearly to be seen by indirect vision when the disk was fixated, I anticipated

difficulty in avoiding an impression which would be constant, and was uncertain if it would be possible by this means to determine whether the eye had moved to make the marks focal, or had merely seen them indirectly. In order to guard against this latter possibility, the instruction as at first used included the sentence: "Pay no attention to the left side of the card."

To my surprise, the marks made no such impression upon the observers. As a rule, they were not noticed at all. When this was discovered the reference to the left side of the card was omitted from the instruction, and the marks were enlarged in size and placed nearer to the disk. But even these changes produced no marked effects upon the results. Three of the observers, M., K. and B., noticed the left side of the card once each. Obs. Av. noticed it twice, Obs. P. 15 times and Obs. Ad. 21 times. Unfortunately, but 5 of the 21 instances recorded by the last named observer were experiments in which an image was secured. These were all of the memory variety, but in each case the noting of the distraction-mark preceded distinctly the formation of the image. They cannot, therefore, be cited as evidence of kinesthesis directly attendant upon the memory-image. Eleven of the 15 instances reported by Obs. P. were also those in which the eyes swept to the left before any image was evolved. Of the remaining four cases in which the consciousness of the distraction-marks appeared to be simultaneous with the production of the image, three were memory-images, and the fourth an image which seemed original at the time of production, but which was afterwards judged to be a recollection. The single case of Obs. M. appeared simultaneously with the production of a memory-image; that of Obs. K. attached to an imaginary production; while with Obs. B. the instance was one in which both imaginary and memory-images were present. Obs. Av. noted the left side of the card once simultaneously with an imaginary production, and again after the appearance of a memory-image as she was reacting.

The conclusions which may be drawn from these few results are not at all decisive, yet the results seem to indicate steady fixation for both kinds of images. This is, of course, attributable in large measure to the specific instruction upon this point,

and shows nothing more than that our conditions were unsuited for studying the possibilities of these eye-movements. But there were indications that a more or less steady fixation was often maintained even when the observer fancied he was looking in another direction. The following report illustrates this:

> Obs. B., 7. *Taft* (4,863σ): "Distinct image at once, spontaneous, not on the disk but a little to the left of me (description follows). Then I thought he was not on the disk, and tried to put him there. He would not go, and I decided to let him stay. . . . Eyes were looking towards the disk at first, rather looked to the left when the image came, and tried to move the image back with my eyes. There was nothing that I know of on the left side of the card."

As a matter of fact, there *was* a distraction-mark at the left, and it is therefore doubtful if the eyes actually moved as the introspection indicates.

The introspective reports were recorded verbatim by the experimenter immediately following the reaction. The observers reported spontaneously upon their experiences. Only after this report was complete did the experimenter venture an occasional question regarding points which seemed to demand more explicit record. In order that these supplementary statements may be held apart from the original report, they are classified and referred to as *secondary introspections*. The points especially stressed by these questions were the following: (1) Was the image from memory, or imagination? (2) How was it localized? (3) Was it colored or uncolored? (4) Was there an affective tone present? (5) Was the image distinct or vague? (6) Did the image appear spontaneously, or was it introduced by a thought or associative tendency of any sort? The observers soon acquired the habit of reporting upon these points spontaneously, so that it was not often necessary to put the questions. Care was taken not to interfere with an observer's natural inclinations in making his report, nor to impress these or other points upon him in a manner which might interfere with, and render artificial, the experiences of the main-period. In this regard, the evidence of the introspections is highly satisfactory. Very seldom did an observer report attempts at introspection during the course of the experiment proper, and all other indications of artificiality were negligible.

The records along the six lines mentioned, together with those for the reaction-times, the frequency of associations, of stability and plasticity in the images, and, finally, a brief study of certain equivocal images, constitute the principal findings to be considered in the remainder of this paper.

III. Results

§ 1. *Images of Memory (M's) and Imagination (I's)*

In the 250 experiments performed, there were reported 251 images. All observers, save one, failed to produce images in some experiments,[1] and all at times reported more than one image to have been aroused. The number of images in one experiment varied from one to three. This doubling or tripling of the images occurred, in all, 40 times, and was distributed among the observers as follows: In the course of 25 experiments Obs. M. reported 2 images 9 times, and 3 images 4 times: Obs. Av., 2 images twice. In the course of 50 experiments Obs. K. reported 2 images 6 times, and 3 images once; Obs. Ad., who reported images in only 19 experiments of the series, found 2 images 4 times; Obs. P., 2 images twice, and 3 images once: Obs. B., 2 images 10 times, and 3 images once.

In most cases there was no difficulty in assigning the image definitely to memory or imagination. A certain number, however, were equivocal. These we shall speak of more in detail in § 9. Our first and main purpose is to consider the correlation of those images which were clearly *M*'s and *I*'s with the data already outlined. The criteria which seemed to be employed primarily by our observers were those of "familiarity" and "unfamiliarity." When the two seemed mixed, or the observer was in doubt, the image was classed as equivocal.

We have not made use of the additional criteria employed by Perky of "particularity" and "personal reference"[2] for *M*'s. With regard to particularity, this term appears too indefinite for use in an unbiased description of experience. The only definite significance which it seems possible to attach

[1] The number of experiments in which the task was accomplished by each observer follows: M., 25; Av., 23; K., 48; Ad., 19; P., 41; B., 48.

[2] *L. c.*, p. 436.

to the term would make it applicable in cases where the image refers to objects which have never been experienced, as well as to those which are remembered. This has already been pointed out by Koffka.[1] He refrains, therefore, from classifying his images as memory and imaginary, grouping them instead into "Individual- und Allgemeinvorstellungen." "Dabei," he says, "verstehen wir unter Individualvorstellungen solche, die einen bestimmten Individualgegenstand (in weitesten Sinne) in irgendeiner Weise repräsentieren; unter Allgemeinvorstellungen solche, deren Gegenstand nicht ein bestimmtes Einzelding, Einzelvorgang usw. darstellt." Inasmuch as our primary interest lies in testing the distinction of *M*'s and *I*'s by reference to certain direct experimental data, it is not in line with our purpose to accept Koffka's classification. Furthermore, the distinction which he makes use of is clearly a matter of intentional or judgmental reference upon the part of the observer, and not a distinction which, in Külpe's words, is "characterized by the appearance of particular series of sensations or ideas."[2] It is sufficient to note that among the 251 images here dealt with, but 15 could be reasonably classed as lacking in that sort of particularity which signifies lack of individuality, and a general, rather than a definite and individual, connotation. These were, quite naturally, imaginary, and in all but one instance vague and indefinite in form. But there also occurred many *I*'s which were as definite and individual as were the *M*'s, and in certain cases they referred clearly to definite objects, as for instance:

Obs. B., 50. *Plato* (5,096σ): ". . . Image of a man, tall, bald in tront of head, dignified broad face and forehead, wearing black suit and long coat. Knew nothing as to the style. Looked as though he were on a stage, or up somewhere. Seen from just a little to the rear and left of him. His forehead was whiter than the rest of his face, and he looked as if he were thinking."

Obs. M., 20. *Khedive* (7,940σ): "'Khedive of Egypt' repeated. Then image of a dusky oriental personage. Turban on head, bright-colored robes, red and white. . . Believe he was seated, but don't know just how. Thought of the Orient. It was in bright sunlight. . . ."

As for "personal reference," this appears also to be ambigu-

[1] *L. c.*, p. 226.

[2] 'Outlines of Psychology.' London: Swan Sonnenschein, 1895, p. 188.

ous, as Koffka has noted.[1] An image may be imaginary and yet possess a personal reference, as in the following case:

Obs. B., 41. *Pencil* (3,926σ): "It was yellow, located on the disk to right of center with the end containing rubber at bottom. The pencil was sharpened and seemed to have been used only a few times. There was no thought that it was my pencil, but there was a thought before that it came out of my pocket, and some connection existing between it (my pocket) and the disk. Then the image came definitely."

Secondary Introspection: ". . . I have no such pencil, though I have had one. The image was not definitely related to any particular experienced pencil. I was conscious of the fact that I did not have a pencil with me. There was some imagery about pocket, which seemed open just a little. It seemed on the wrong side, and I wanted to feel for it. . . ."

It is of course true, at least so far as our reports are concerned, that *M*'s are most frequently of particular things, with particular context and a personal reference, while *I*'s are apt to lack these characteristics. Yet as Koffka has shown, and we can confirm, this is not necessarily the case. Therefore, these additional criteria cannot be regarded as absolute or final.

Reference may also be made at this point to a further criticism which Koffka has passed upon the Perky results.[2] He maintains that an instruction to secure images, such as we, too, have used, is not advisable at present, since in this early stage of exact investigation we ought to direct our inquiries to the differentiation of as many factors as possible, rather than to attempt an exact description of some certain one. He argues, therefore, that his own data, secured with a more general instruction, are more reliable with reference to the natural conditions of the imagery aroused. We do not find, however, that this criticism is sufficiently weighty to invalidate our results. It is true that our instruction tended to emphasize the image, but except for the fact that it was to be a *visual* image, our instruction does not differ materially from that which Koffka, himself, has employed in his first series of experiments.[3] Furthermore, the general agreement which we are able to establish between our results and his, so far as they touch upon the same points of inquiry, guarantees to a certain extent that our images were not, as compared with his

[1] *L. c.*, p. 231.
[2] *L. c.*, p. 190 f.
[3] *L. c.*, p. 27.

own, either more artificial, or more constrained by the special instruction.

Of the 251 images reported, 138 were *M*'s, 75 were *I*'s and 38 were equivocal. The individual distribution follows:

TABLE I

Obs.	*M*'s	*I*'s	Equivocal Images
M.	30	12	0
Av.	10	8	7
K.	25	18	13
Ad.	15	7	1
P.	30	7	8
B.	28	23	9

It will be noted that *M*'s were nearly twice as frequent as *I*'s, and in but one individual case, Obs. B., do the two classes approach in frequency. Perky reports a preponderance of *I*'s in her results with visual imagery,[1] but she fails to give her lists of stimuli-words, and the individual distribution of the imagery. Martin remarks that "im Allgemeinen war das Gedächtnisbild beliebter als das Phantasiebild,"[2] but she, too, gives no exact data regarding number and distribution. In our experiments the stimuli-words were selected, as already noted, with a view to securing an approximately equal distribution of *M*'s and *I*'s. A comparative study of the reports, however, indicates that agreement was hardly the rule. With the following 13 words, *M*'s were preponderant: *Diver*, *Viaduct*, *Dean*, *Slippers*, *Market*, *Father*, *Dr. Ayres*, *Bijou*, *Mother*, *Roosevelt*, *Post-Office*, *Rink* and *Co-ed*. With the following 6 words, *I*'s were the rule: *Lyre*, *Duke*, *Druid*, *Khedive*, *Flora* and *Pegasus*. With the remaining words no such regularity was to be noted.

§ 2. *Localization of the Image*

The instruction did not call expressly for the projection of the image, but a suggestion to this effect was given in the direction that the disk should be fixated. Our results indicate that projection under such conditions is both natural and easy.[3] Only one observer (P.) ever failed to project an image, and this

[1] *L. c.*, p. 442.
[2] *L. c.*, p. 391.
[3] Cf. Martin, *l. c.*, p. 391.

occurred with him but five times. In these cases he described the images as being "in his head." Three of these were *M*'s, one an *I* and one equivocal.

It was a matter of surprise to the experimenter that the favored location was not that of the disk. One observer (K.) marks an exception. Her images were invariably upon the disk. Obs. M, however, placed none there; Obs. Av., 2; Obs. Ad., 8; Obs. P., 15; and Obs. B., 24. This result appears to be at variance with Martin's statement[1] that all her observers agreed that the direction of the eyes and attention upon a certain position in space favored the location of the image in that place. She does not, however, assume this to be the all-sufficient ground for localization, but notes, among others, the following conditions as being also involved: (1) whether the image is so constituted that it is suited to projection in the space fixated; (2) if its size is such that this would be possible; and (3) if its connection with its natural environment is such that it can be readily separated therefrom to permit such a projection. In the case of Obs. K., who projected all her images upon the disk, difficulties of this sort were seldom experienced. In four cases the image seemed to extend somewhat beyond the limits of the disk, though no alteration in distance was reported. In two of these cases the environment appeared to require the extension. These were *M*'s in a definite setting. The other two were *I*'s, although one of them bore some relation to a picture previously seen. One reason why the conditions mentioned by Martin were inapplicable in our work may be found in the fact that the disk was so close to the eyes of the observer that, although small, it was yet large enough to permit the projection of even somewhat extended scenes.

In classifying the localizations, other than the one mentioned, consideration was given to the schemes used by Martin[2] and Koffka,[3] as well as to the interesting suggestions of G. E. Müller.[4] All these authors have emphasized the importance

[1] *L. c.*, p. 501.

[2] *L. c.*, p. 465 f.

[3] *L. c.*, p. 219 ff.

[4] 'Über die Lokalisation der visuellen Vorstellungsbilder. Bericht über den V. Kongress für experimentelle Psychologie,' *Arch. f. d. ges. Psychol.*, 1912, *24*, 73–76.

of distinguishing an objective and a subjective component in localization, but their interpretation of this distinction varies. Martin gives twelve different classes as dictated by the analysis of her results. Her guiding principle is the observer's apparent location. In Class I. the observer is transported to the place where the object imaged is located. In Classes II.–X. the observer retains his position in the experimenting-room, and the image is projected (II.) at a distance, (III.) in the surrounding space, (IV.) upon the wall, (V.) behind him, (VI.) in an indefinite space, and (VII.–X.) in various parts of his own head. Class XI. deals with cases in which observer, or image, or both, change their positions during the experiment, and Class XII. includes those cases in which neither the observer nor image was definitely placed.

Koffka makes use of but three localizations: (1) The observer remains in his present position, the image is seen at a certain distance. (2) The observer occupies his present position, and the image is seen at a distance, yet the relation of the observer to the image is not established. In other words, the observer realizes the direction of the image, but it bears no relation to the observer's space. (3) The image and observer are both projected at a distance. It will be seen that group 3 is evidently identical with Martin's Class I.; group 2 coincides fairly with Class VI., and group 1 may embrace Classes II., III. and IV. The objective component of this grouping is not as explicit as in Martin's classification, yet it serves to emphasize the importance of different subjective relations to the image as they were indicated in experiments in which no special instruction was given bearing upon localization.

Müller appears to approach the problem from a rational point of view, with the purpose of defining all possible localizations. The objective component for him has reference to the particular place in which the object is conceived to be. This he calls *topomnesic* localization. The subjective factor concerns the apparent spatial relation of the object to the observer. He classifies localization as follows:

I. The object is conceived in the place in which it was originally perceived. The subjective components may be

three in number: (1) The observer appears to be in the original position of the perceiver (conservative-topomnesic localization). This combination seems to correspond to Martin's Class I. (2) If the original position of the observer was unusual, it may be modified to a more habitual position (habitual-topomnesic localization). (3) If the observer, in reproducing the image, is near the object, but in a position which differs from the one originally occupied, he may project the image in the direction and distance in which he knows the object to be (egocentric-topomnesic localization). This coincides in part with Martin's Class II. Because of the emphasis upon maintenance of the original position of the object, it is difficult to correlate Koffka's third group with Müller's topomnesic localizations, though many of Koffka's images would doubtless belong here.

II. A purely egocentric localization, in which the original objective component is not preserved, and the subjective component may be either conservative or habitual. Three different egocentric systems may here be involved in determining the distance, direction and orientation of the object: (1) a system dependent upon the position of the head (*K-system*); (2) a system dependent upon the three axes of spatial projection by reference to the line of regard (*B-system*); and, (3), a system dependent upon the normal position of the trunk (*S-system*). The subjective component may be conservative or habitual in each of these systems. These localizations correspond fairly well with Martin's Classes II.–V., and VII.–X., although her classification does not refer to the coördinations of head, line of regard and bodily standpoint. They will also include Koffka's first group.

III. Here neither the topomnesic nor the egocentric localization tendencies are effective, and the localization is definite only with reference to the subjective component as being conservative or habitual. The object is located from an objectively indeterminate standpoint in a subjectively determined manner. This localization corresponds closely with Martin's Class VI., and with Koffka's second group.

IV. Indeterminate localizations, which may, however, be only partial, in that the orientation, as above, below, front,

back, may still be determined. This is somewhat more inclusive than Martin's Class XII.

In evaluating this classification for visual images generally, the question arises if it is always clearly applicable for *I*'s which have no definite topomnesic setting, and in which the subjective component may not be determined in the exact manner which these classes suggest. It may be hoped that experiments will soon be undertaken in which this thoughtful attempt to systematize spatial localization is subjected to a thorough test, in order that we may arrive at a standardized procedure in dealing with this problem.

For our present purpose, none of the above schemes is altogether applicable, partly because of the fact that when the experiments were performed sufficient care was not taken to secure definite information upon all the points noted by these writers. But furthermore the fact that our observers were instructed to fixate the disk resulted in localization by reference to the disk more frequently than with regard to subjective and objective components. The observer did not always indicate in the case of a distant projection if he was himself on the scene in which the image appeared, or if his position as observer remained unchanged. It was also difficult in some cases to determine the exact topomnesic value of images which were projected beyond the immediate environment of the observer. It seemed best for this study, therefore, to relinquish the important point of determining the precise relationship of the objective and subjective components, and to base our classification with reference to the disk as standard. We have then, in addition to this, localization at a distance, usually topomnesic; localization in near-by space, between the observer and the disk, through the disk, to right or left, above or below, in front or back of the observer; and finally, an indefinite group. These four classes may be summarized as follows:

I. Localization upon the disk.

II. Localization off the disk, but in the surrounding space.

III. Localization at a distance, usually in the object's proper environment.

IV. Indefinite localization.

The following table indicates the distribution of *M*'s and *I*'s for these different localizations:

TABLE II

Obs.	Group I				Group II				Group III				Group IV			
	Image *M*		*I*		*M*		*I*		*M*		*I*		*M*		*I*	
	No	Per Cent.	No.	Per Cent.	No.	Per Cent.	No.	Per Cent.	No.	Per Cent.	No	Per Cent.	No.	Per Cent.	No.	Per Cent.
M.....	0	0	0	0	10	33.3	9	75	15	50	2	16.6	5	16.6	1	8.3
Av.....	1	10	1	12.5	1	10	3	37.5	8	80	4	50	0	0	0	0
K.....	25	100	18	100	0	0	0	0	0	0	0	0	0	0	0	0
Ad.....	1	6.6	7	100	7	46.6	0	0	2	13.3	0	0	5	33.3	0	0
P.....	11	36.6	4	57.1	7	23.3	1	14.2	4	13.3	0	0	8	26.6	2	28.5
B.....	4	14.2	20	86.9	5	17.8	2	8.6	17	60.7	0	0	2	7.1	1	4.3
Total .	42	30.5	50	66.6	30	21.7	15	20	46	32.3	6	8	20	14.5	4	5.3
	(Total *M*'s and *I*'s together)															
	92	43.1			45	21.1			52	24.4			24	11.2		

The percentages for the individuals are based upon the number of *M*'s and *I*'s, respectively, which an individual secured. For the totals, they are based upon the complete number,—namely 138 *M*'s and 75 *I*'s, in the first instance, and 213 images in the second.

It will be seen at once that both *M*'s and *I*'s are projected into each of our four localities. There appears to be nothing inherent in an *M* or *I* which makes it impossible to project it either on the disk, in the surrounding space, at a distance, or indefinitely. Yet when we study the record more closely, we discover rather well defined tendencies which seem to indicate correlations with memory and imagination, respectively. Location I., which found no instances with Obs. M., and but two with Obs. Av., comprised all the images of Obs. K., and is favored rather decidedly by the remaining three observers as a locus for *I*'s. Location II. is the favored locus for *I*'s with Obs. M., and also to some extent for Obs. Av., but the number is here too small to be significant. Obs. Ad., P., and B. favor it for *M*'s, though strikingly so only in the case of the first-named observer. Location III. is favored for *M*'s in all cases, except, of course, Obs. K. Location IV. is more ambiguous. Individual differences evidently play a considerable

rôle, and, of course, the total number of images is too small for significant results, but the tendency for *I*'s to be located upon the disk, and for *M*'s to be located off the disk is well marked in the total results. This agrees with Perky's statements regarding localization. *I*'s, she says, were usually "on, or at the exact distance of the wall." *M*'s "as a rule appeared to be some distance away, often approximating the distance of the remembered object for the observer at the time of observation."[1] It is to be regretted that Perky did not record the data upon which her conclusions were based. Koffka notes that the *I*'s are not necessarily lacking in definite orientation,[2] which we too have found to be true. It follows accordingly that the distinction between *M*'s and *I*'s cannot be said to rest upon such data as we have here collected. As a secondary criterion, however, the fact that these tendencies should be more or less habitual merits some consideration.

In this connection, Martin notes the advantage of her projection-method, which required the observer to project *M* and *I* side by side, thus placing both images under the same conditions and in the same location. "Dass die Verschiedenheit der Lokalisation an sich einen Unterschied zwischen Gedächtnis- und Phantasiebildern sehr wohl bedingen kann," she adds, "wird aus den Erörterungen des zweiten Teiles sich ergeben."[3] So far as I have been able to detect from a perusal of her work, this diversity in localization seems to rest principally upon the selection of a suitable location for the image, although she also finds that the surface upon which the image is intended to fall frequently influences the nature of the image,[4] and, in addition, lists a summary of 22 factors which may contribute to localization.[5] I have, however, failed to find any direct reference to a distinction of *M*'s and *I*'s based upon localization alone.

With three of our observers instances were noted in which an image shifted its position during the course of an experiment,

[1] *L. c.*, p. 445 f.
[2] *L. c.*, p. 239.
[3] *L. c.*, p. 397.
[4] *L. c.*, p. 496.
[5] *L. c.*, p. 511 ff.

or an attempt was made to shift it to the disk. For Obs. Av. we have three relevant experiments:

11. *Watch* (2,226σ): ". . . First it was somewhere before me, then it was right by the disk. A clear image, but did not seem to get on the disk. It seemed in front of the disk, between me and it. The size was the same when at a distance and when near."

This was a memory-image of the observer's own watch, yet it was not definitely recognized at the time.

12. *Duke* (2,586σ): "Image of an armed man, not very clear, seen at a distance." Then,—"stood right over the disk, and became more like a picture, but did not fit on the disk. Head and shoulders were above, and feet below. . . . The figure was less clear and smaller when it got to the disk. The first image was life-sized."

19. *Father* (2,703σ): "Thought of fathers as heads of families. There may have been an image of men's heads. Then, quickly, an image of my own father. Position not clear. At first down in front, then at a distance back, in location of home as I thought it (incorrectly). Yet I was facing the image all the time. . . . Then I remembered to see the image on the disk, but could not get it clear. It came before the disk about 3″ tall, not touching the disk. The left side was clearer than the right, only a rim was visualized, as though there was no substance in the center where the disk was seen."

These all indicate a certain inertia of the image, whether it be *M* or *I*, which makes it difficult to remove it from its original position to the disk.

This inertia is even more clearly shown in the reports of Obs. B. In 7 experiments images were reported which "did not want to go on the disk." Four of these were *I*'s in a remembered or familiar setting, and reckoned, accordingly, among the equivocal cases. Two were *M*'s and one an *I*, although this (29, *Glasses*) was closely associated with a glimpse of the observer's own spectacles. He says of this: "I think I was trying to put my own glasses on the disk. The image was rather vague and hard to put there. It seemed to want to go between the disk and my eyes." In one instance (19, *Father*) an *I* of church father came down off the disk, while an *M* of observer's own father came up "about as high as the disk, but had no definite location as to floor, etc." In four further instances, all *M*'s, the image was first formed on the disk, but then came off it. Four images were reported as being "carried to the disk." Two of these were *I*'s, one was equivocal—an *I* which was in some way connected with memory—and the fourth was an *M*. In this last case, however, it was difficult to

place the image on the disk. "I saw it, rather," he says, "*through* the disk."

Obs. B. also reported four *M*'s which were dynamic, seeming to shift their topomnesic positions in the course of the experience. None of these was at any time located upon the disk.

Finally, Obs. Ad. reported difficulty in placing *M*'s upon the disk three times. In the first of these instances (4. *Hamlet*, 10,498 σ) an *I*—"slight image of the angle of a roof"—preceded and associated an *M*—"a painting of a house of this sort." The *I* appeared upon the disk, but the *M* was topomnesic. The second instance (6. *Castle*, 28,042 σ) hardly warrants special comment; the image would not appear definitely upon the disk, though it was evidently present. The third instance is more ambiguous, yet it presents some points of special interest, so we give the introspection in full:

9. *Viaduct* (13,620σ): "I saw the word first, vaguely saw the disk and, to the left, the blankness of the card. The word suggested a French word—remembered I had used it last night in translating a story I wrote into French. 'Viaduc' spoken but not correctly pronounced. Was conscious that there should be some upright concrete pillars as supports, and across these the horizontal part. *Did not get that image at all.* Just then I seemed to lower my head a little so that the rim of my eye-glasses doubled the edge of the disk. About the same time I took another position as though I were on the driveway and looking down the length of the viaduct. *There must have been something visual at first, for it took me a while to know what it meant.* This looking down across the top never became very definite. I reacted then."

Secondary Introspection: "It was the viaduct of the story, slightly modified. With the second thought, the image was above the center of the disk, back to right, front to left in extension. It was the Asylum Avenue viaduct, I knew this all the time. The image was non-spontaneous, introduced by the thought of the story. I think there were some indefinite visual images of writing. No especial images connected with writing the story which were important. At beginning of introspection had a clear image of a page with writing, one word underlined. No emotional distinction noted, though it was my story."

Two interesting facts may be noted in this report: (1) that the data regarding the viaduct as *at first* thought of did not become imaged,[1] and (2) that the image which came was so

[1] That the presence of a thought does not necessarily condition the nature of the image which follows it, is also revealed in other reports which we have secured. The following, from Obs. P., is a case in point: 35. *Roosevelt* (2,608σ): "First an idea of his characteristic slouch hat, glasses and moustache. No image until these ideas left, then an image of him standing on the steps of the Main Building at the Exposition. . . ."

Secondary Introspection: "No words or images were attached to the ideas, yet there was a conscious attempt to put them into an image. The attempt failed, and the image came spontaneously when they disappeared."

different from what had been expected that its meaning was not at once grasped. This is an interesting evidence that the content was imaginal. Regarding its location, there is less certainty. At least, there was a definite thought concerning the real location of the object.

All these results emphasize the tendency, already noted, to locate *M*'s off the disk, together with the inertia which they evidence when an attempt is made to bring them upon the disk. The *M*'s were more largely concerned in these cases than the *I*'s, yet it is worthy of note that we have found but one instance in which difficulty was encountered in the projection of an unequivocal *I* upon the disk (the one cited for Obs. Av., 12, *Duke*).

§ 3. *Color in the Images*

The presence or absence of color in the images did not reveal any significant correlations with *M*'s and *I*'s. The following table indicates the results of this analysis:

TABLE III

Obs.	*M*'s		*I*'s	
	No.	Per Cent.	No.	Per Cent.
M.	22 (30)	73.3	16 (22)	72.7
Av.	7 (9)	77.7	5 (6)	83.3
K.	12 (22)	54.5	4 (14)	28.5
Ad.	3 (15)	20.0	0 (7)	0
P.	13 (25)	52.0	2 (4)	50.0
B.	14 (15)	93.3	19 (21)	90.4
Total.	71 (116)	61.2	46 (74)	62.1

The number refers to the cases in which color was reported. The percentage is based upon the total number of cases (*M*'s or *I*'s) in which the judgment was made. These numbers appear in parentheses.

With the exception of Obs. Av., the individual percentages favor the *M*'s, though very slightly. Perky, while she gives no statistical summary upon this point, remarks that *M*'s were in content "colorless etchings," while the *I*'s were "oftentimes, on the contrary, very highly colored."[1] Our data do not warrant such a distinction. Martin's results are here more

[1] *L. c.*, p. 447.

in accord with our own. In discussing the difference between *M*'s and *I*'s with reference to color and certain other perceptual factors, she remarks: "Im Hinblick auf die hier genannten Punkte lässt sich in den Aussagen der Beobachter nicht ein einziges unbedingt zuverlässiges Kriterium der beiden Bildgruppen nachweisen."[1]

§ 4. *Affective Tone*

Our results on this question are principally negative. Although care was taken to make frequent inquiry concerning the arousal of feeling of any sort, it usually met with a negative response. It occurs to the writer that this may in part be due to the conditions of the experimental method employed. The buzzing of the clock and the quick exposure of the stimulus are conducive to a rather high state of tension on the part of the observer. He is keyed to a rapid reaction, preceded by a consciousness which is distinctly parsimonious, engaging itself, in the main, very actively with the particular problem at hand. Thus, the more subtle shades of affection which the experience arouses may go unnoticed in the general stress which is laid upon the solution of the problem. The question as to the possible effectiveness of such unnoticed feelings is one which we cannot here decide.

Two of our observers (Av. and P.) reported no feelings at all. The remaining four reported nine instances, eight of which attended *M*'s. Obs. M. reported a "feeling of familiarity and love" preceding the image which was aroused by the word *Father*. Obs. K. reported "amusement" accompanying the appearance of an image of Mephisto, derived from a label for potted meat, and called out spontaneously by the stimulus word *Hamlet*. She also reported a "feeling of familiarity" and a "feeling of relationship" attendant upon the stimuli *Mother* and *Sister*, respectively. Obs. Ad. reported "feelings of familiarity" with *Market*, referring to the local market-house region, and *Station*, referring to the railway-station at his home. Also, a feeling-tone of "something terrible" was present with the pronunciation of the stimulus *Dragon*, followed by an *M*,

[1] *L. c.*, p. 387.

and a disagreeable tone attached itself to the stimulus *Pegasus*, although this was quite independent of the image and its meaning, which appeared later. Obs. B. reported a "home atmosphere, pleasant and responsive," attendant upon an image aroused by *Sister*.

No evidence was given of the distinctive "mood of surprise" and "mood of recognition" which Perky found attached to the *I*'s and *M*'s, respectively, in her experiments.[1] So far as our data are concerned, there was no surprise with *I*'s, and as for recognition, while doubtless it was an important ground for discriminating *M*'s from *I*'s, it appeared more often as a rational process of judgment, than as a mood or feeling.

§ 5. *Distinctness of the Image*

Although the instruction called for an image, "clear and definite," this was not always the result. Judgment was made upon 196 of the 213 *M*'s and *I*'s, resulting in 131 distinct and 65 vague images. Finer distinctions in point of gradation were often made, but as this was not impressed systematically upon the observers, it does not seem wise to undertake a more exact analysis than the one noted. The following table records the individual distribution of these judgments in the same manner as in the preceding tables:

TABLE IV

Obs.	*M*'s		*I*'s	
	No.	Per Cent.	No.	Per Cent.
M.	23 (28)	82.1	8 (12)	66.6
Av.	6 (8)	75.0	3 (7)	42.8
K.	16 (24)	66.6	8 (16)	50.0
Ad.	5 (16)	31.2	1 (7)*	14.2
P.	21 (24)	87.5	4 (7)	57.1
B.	21 (25)	84.0	15 (22)	68.1
Total.	92 (125)	73.6	39 (71)	54.9

It will be seen that all observers indicate a greater tendency towards distinct images among *M*'s than *I*'s. In some cases, the number of judgments is too small to be significant, yet the

[1] *L. c.*, p. 442.

general agreement among the observers has a certain weight, and therefore may be said to call in question the universal application of Perky's conclusion that *M*'s are "scrappy" and "filmy," while *I*'s are "substantial" and "complete."[1] To be sure, the criteria mentioned by Perky are not identical with the one here dealt with. We shall speak later (§ 8) of stability and plasticity in the image. But so far as distinctness is concerned, our *M*'s were not usually "filmy," nor our *I*'s "incomplete." On this point we are better able to support Martin's results.[2] One fact is to be noted in this connection which doubtless renders the results obtained by Martin's method of direct comparison more reliable than those which we have obtained. Where a direct comparison is impossible, the norm of distinctness is not only a very uncertain quantity, but the judgment is likely, at times, to be biased by conditions which do not properly belong to the visual content, as such. For instance, the notion of "completeness," "knowledge about," or "familiarity with," the object imaged, are, in the opinion of the writer, imageless as to content, consisting in elements which are distinct from those which go to constitute the image, strictly speaking.

§ 6. *Spontaneity of the Image*

The question raised here concerns the mediacy or immediacy of the image. Among the 186 cases in which judgments upon this point were made, 82, or 44 per cent., were spontaneous images. This does not necessarily mean that these images occurred at once upon the exhibition of the stimulus-word, although this was usually the case. It does mean that no mental operation was reported, as preceding the image, which could be ascribed as introductory to it.

The following table gives the results of this analysis with reference to the images reported as spontaneous:

It will be noted that, on the whole, *M*'s appear to be more often spontaneous than *I*'s, which agrees with Martin's results.[3]

[1] *L. c.*, p. 451.
[2] *L. c.*, p. 401.
[3] *L. c.*, p. 386.

TABLE V

Obs.	*M*'s		*I*'s	
	No.	Per Cent.	No.	Per Cent.
M.	17 (18)	94.4	6 (7)	85.7
Av.	5 (9)	55.5	4 (6)	66.6
K.	16 (23)	69.5	7 (16)	43.7
Ad.	1 (14)	7.1	2 (9)	22.2
P.	10 (26)	38.4	0 (7)	0
B.	10 (28)	35.7	4 (23)	17.3
Total.	59 (118)	50.0	23 (68)	33.8

The case is, however, not a very strong one, as a consideration of the individual records will show. The first three observers all tended towards spontaneous imagery, and the difference in the appearance of *M*'s and *I*'s is not striking, except, perhaps, in the case of Obs. K. The last three observers, on the contrary, tended strongly towards mediated imagery and, with the exception of Obs. Ad., whose results were too few to be significant, found *M*'s decidedly more spontaneous than *I*'s. Individual differences thus appear to play a large part in the production of the image. The peculiar conditions of these experiments seem, for the first three observers, to have been conducive to immediate, spontaneous imagery in both classes, whereas for the other three they appear to have occasioned frequent inhibition. Under other conditions, in which the observers are not constrained by so strong a reaction-tendency, the results might conceivably be quite different. We should hardly be justified, therefore, in concluding that the greater frequency of mediated imagery of the *I* type is evidence for the assumption, sometimes made, that imagination necessarily involves an act of will as its conscious antecedent.

§ 7. *The Reaction-time*

In tabulating the reaction-times we have omitted those experiments in which more than one image was present, in order that the results might gain as great a degree of uniformity as possible. It may be noted, however, that a comparison of the average results for an observer with the excluded cases of the same category did not indicate that the presence of more

than one image has the effect of prolonging the reaction. On the contrary, this was noted to be the case in but three instances out of the 18 which were rejected.

These time-results comprise, in all, 94 reactions, 62 of which were to *M*'s and 32 to *I*'s. The average time of the former was 4,318 σ, and of the latter 6,097 σ. The individual results are recorded in the following table:

TABLE VI

Obs.	*M*'s				*I*'s			
	No.	*A*	*M*	*mv*	No.	*A*	*M*	*mv*
M.....	7	1,176.2	884	504.1	2	985.5	985.5	191.5
Av.....	8	2,155.0	2,193.5	202.7	3	2,433.6	1,980.0	604.8
K.....	9	2,610.3	1,871.0	1,166.6	9	2,382.0	1,888.0	840.6
Ad.....	9	9,207.6	7,321.0	4,404.8	5	13,265.4	13,343.0	2,560.4
P.....	21	4,064.6	3,897.0	916.2	5	8,839.8	8,667.0	2,764.5
B.....	8	6,315.8	4,608.5	3,039.0	8	6,735.1	6,163.5	1,661.8

This table indicates the number of reactions, the arithmetic mean (*A*), the median (*M*) and the mean variation (*mv*). The reaction times are given in *sigmas* (thousandths of a second).

The most striking result of this comparison is the very general agreement of the individual observers with the average of the total reaction-times, which we have noted as evidencing a shorter reaction-time for *M*'s than for *I*'s. The only exceptions are found in the cases of Obs. M. and K. In the first instance, we find the median to be smaller for *M*'s than *I*'s, which points to one reaction, the longest obtained by this observer. Although we have no indication from the introspection that this was unusual, we are perhaps justified in excluding it, since it was the second reaction of the series, and this observer had performed no preliminary experiments. If we do exclude it, the average for the remaining six *M*'s is 883.8 σ as compared with 985.5 σ for the two *I*'s. In the case of Obs. K. the exception is as readily accounted for. The larger average for *M*'s is again due to the presence of one exceptionally long reaction (4. *Hamlet*, 6,119 σ). If this be eliminated, the average for the remaining eight is 2,171.7 σ, which is slightly less than the average for *I*'s (2,382 σ). The introspection of this particular experiment justifies us in holding it to be an unusual case. The

observer reports that the image "did not come for quite a while." Before it appeared there was a blank, and "some thought that the word referred to an actor," also "that the figure should be in black." Such an image did not appear. The one which finally came was of Mephisto in red, which aroused a sense of incongruity and surprise, followed by amusement. Later the image was recognized as "connected with some sort of advertisement, seen somewhere for potted-meat." This was the longest reaction-time which occurred with this observer in any experiment. The reactions which most nearly approach it are two, 4,465 σ and 3,580 σ among the *I*'s; and one, 4,335 σ, among the *M*'s. All the other records were less than 3″.

We may therefore conclude that our observers, individually as well as collectively, indicate a "temporal course" which is at variance with that reported by Perky in her statement that "the images of imagination appear more quickly, more suddenly . . . than the images of memory."[1] Our results agree better with those of Martin, who reports for three observers who were endeavoring to project *M*'s and *I*'s simultaneously, that the *M* usually appeared before the *I*.[2]

In considering the spontaneity of images in the preceding section, we noted that this usually indicated the appearance of an image at once upon the fixation of the disk. One might expect that images of this sort would give shorter reaction-times than those which are mediated by an introductory thought. This, however, does not appear to be necessarily the case in our results. With Obs. M., all images for which the reaction-time was considered were of the spontaneous variety, whereas with Obs. Ad. none was spontaneous. For Obs. Av. there were 5 spontaneous *M*'s, averaging 2,113.8 σ, and 3 non-spontaneous *M*'s, averaging 2,223.6 σ. Among the *I*'s there are but two records, each by chance 1,980 σ, one of them spontaneous and the other non-spontaneous. For Obs. K., 6 spontaneous *M*'s are recorded, but three of these were delayed in their appearance by preceding, but irrelevant,

[1] *L. c.*, p. 449.
[2] *L. c.*, p. 401 ff.

thoughts. The 3 which were direct average 2,242.6 σ; the 3 which were delayed average 3,934.3 σ. Three records for non-spontaneous *M*'s, however, average only 1,654 σ. Among the *I*'s, 4 records were spontaneous, but two of these were delayed. The direct images average 4,022.5 σ, the delayed two are shorter, 1,542 σ and 1,533 σ. The non-spontaneous *I*-records are four in number, averaging 2,031 σ. From this we see that Obs. K. had shorter reaction-times for non-spontaneous imagery than for spontaneous. The number of cases is much too small to render this fact of any great importance, but it may serve to remind us that the reaction-time in these cases measures a complicated group of experiences, and as such cannot be considered as an adequate measure for the particular experience which the instruction has emphasized.

For Obs. P., 7 spontaneous *M*'s averaged 3,703 σ, and 10 non-spontaneous *M*'s, 4,610.9 σ. The *I*'s, for which time-records were secured, were all non-spontaneous. For Obs. B., finally, three records for spontaneous *M*'s averaged 3,821.6 σ, and five for non-spontaneous *M*'s, 7,812.4 σ. Among the *I*'s, but one record is given for a spontaneous image, 8,992 σ, while for seven non-spontaneous *I*'s the average was 6,412.7 σ.

§ 8. *Associated Matter, Stability and Plasticity of the Image*

There remain for consideration three factors which are mentioned by Perky and Martin in connection with the distinction of *M*'s and *I*'s. These are the presence of associative data, the stability of the image and its plasticity or solidity. Perky's results indicate for *M*'s a "mass of associative material."[1] They were "scrappy and fleeting,"[2] and unsubstantial. For *I*'s, the opposite conditions seemed to prevail: associations were inhibited, the images were stable and substantial. Martin also found associations attending the *M*'s, though also, occasionally, with *I*'s.[3] *M*'s appeared to be more stable than *I*'s,[4] but not always. Plasticity is mentioned as a possible criterion, but her results do not indicate that any significance could be attached to it.[5]

[1] *L. c.*, p. 451.
[2] *L. c.*, p. 447.
[3] *L. c.*, pp. 408 and 412.
[4] *L. c.*, p. 407 f.
[5] *L. c.*, pp. 384 and 387 f.

Our own results with reference to stability and plasticity of the image are so few that we can attach no great importance to them. No special questions were directed upon these points, and consequently such records as appear in the protocols are those of chance occurrence. Few as they are, however, they may be worthy of a brief consideration, if only by reason of the fact that they are so entirely free of any intentional bias. We therefore append the results in the two following tables:

TABLE VII

STABILITY

Obs.	*M*'s		*I*'s	
	No.	Per Cent.	No.	Per Cent.
M.	3 (5)	60.0	1 (1)	100.0
Av.	1 (1)	100.0	1 (2)	50.0
K.	6 (19)	31.5	3 (12)	25.0
Ad.	2 (3)	66.6	5 (7)	71.4
P.	3 (9)	33.3	1 (2)	50.0
B.	3 (6)	50.0	11 (12)	91.6
Total.	18 (43)	41.8	21 (36)	58.3

TABLE VIII

PLASTICITY

Obs.	*M*'s		*I*'s	
	No.	Per Cent.	No.	Per Cent.
M.	0 (0)	0	0 (0)	0
Av.	0 (0)	0	1 (1)	100.0
K.	2 (5)	40.0	2 (3)	66.6
Ad.	3 (3)	100.0	3 (6)	50.0
P.	0 (0)	0	0 (0)	0
B.	1 (1)	100.0	1 (2)	50.0
Total.	6 (9)	66.6	7 (12)	58.3

The figures given in parentheses after the number of occurrences refer to the total number of judgments made upon the point in question, and serve as the bases for computing the percentages.

These results appear to indicate a greater stability for *I*'s than for *M*'s, in agreement with Perky, although two observers mark exceptions to the general average. Of these, the first (Av.) made too few judgments to be of any significance. The

second (K.), however, furnished more judgments upon this point than any other observer. With reference to plasticity, by which we mean that the image seemed to be solid, or possessing three dimensions, the positive judgments appear to favor *M*'s rather than *I*'s, the same two observers, however, marking exceptions. Yet the total number of cases is very small, and the difference in the percentages of the total results is not great.

In the case of associative material, our data are more satisfactory, for here it was possible to determine in every case whether or not associations were present. The nature of these associations was, in the main, of the imageless variety. The observer often thought about the meaning of the word before the image came, or he would elaborate upon it in thought terms after it had appeared. We have enumerated in our reports all the visual images which occurred, and we have not here regarded the appearance of more than one image in a given experiment as necessarily an evidence of association. But we have attempted, wherever possible, to trace the associations which attached to each image. In a large number of cases successive images were not, in fact, associatively connected, but each appeared independently as a result of the task set by the instruction and stimulus-word. Whenever an associative connection could be traced between successive images, it was, of course, recognized, and the association attributed to each of the images involved.

The following table comprises these results:

TABLE IX

Obs.	*M*'s		*I*'s	
	No.	Per Cent.	No.	Per Cent.
M.	14 (30)	46.6	8 (12)	66.6
Av.	9 (10)	90.0	8 (8)	100.0
K.	9 (25)	36.0	4 (18)	22.2
Ad.	15 (15)	100.0	5 (7)	71.4
P.	24 (30)	80.0	7 (7)	100.0
B.	11 (28)	39.2	18 (23)	78.2
Total.	82 (138)	59.3	50 (75)	66.6

The total results indicate, in opposition to the findings of

Perky and Martin, that associations were more frequent with *I*'s than with *M*'s, the individual results of two observers marking exceptions. These were Obs. K., who reported fewer associations than any other observer, and Obs. Ad., who reported them in all but two of the experiments considered. The difference in the percentages for the total results, however, is not very great, and we cannot conclude, therefore, that our result has much positive value. Yet it may, perhaps, serve to indicate that the opposite assumption to the effect that associations are naturally more prevalent with *M*'s than with *I*'s, finds no support in our results.

§ 9. *The Equivocal Images*

Before bringing our results to a close, it will be in order to say a few words regarding the equivocal images, which we have excluded from the previous accounts. These, as already noted, occurred in all 38 times, and were distributed as follows: Obs. M., none; Obs. Av., 7; Obs. K., 13; Obs. Ad., 1; Obs. P., 8; and Obs. B., 9.

Twelve of these appeared as *M*'s which were too general or too uncertain in their reference to be definitely classified. In one case (Obs. K., 5, *Pen*) an image occurred which merely "seemed familiar." More often the image recalled something, the observer could not say exactly what (five times). Again, the image recalled a definite thing, but was in some way altered: Obs. K., 6, *Castle*,—"this view was solid"; and 42, *Hun*,—"it was colored, the original was not"; Obs. Ad., 7, *Taft*,—"it was strikingly like a picture I took of Roosevelt." Twice, for Obs. P., the image was based upon a generalized memory, no specific object being in mind, and once for him the image seemed to be remembered and yet modified by imagination.

The remaining 26 images all appeared as *I*'s, and were judged equivocally for various reasons. In five instances the *I* was in a remembered setting. Four times it referred to something known, but the exact reference was either not made definite, or it was not based upon first-hand experience, as, for instance, Obs. B. (26, *Griffon*) imagined a town which he knew about, but had never seen, and Obs. Av. (3, *Bed*) imagined the

beds in a certain dormitory which she had never visited. The most frequent cause for ambiguity was attributable to the association of memory-factors, such as the similarity with some familiar object or event (eleven times). Once only was an image made familiar by the appearance of a second image, and this case is rather uncertain: Obs. K., 47, *Rink* (1,457 σ) "Double image, the first very vague and brief, a ring of gray; the second, roof of the Rink." Secondary Introspection: "Meaning not known until the first image came, then it was familiar. A clear idea with the second image. The recognition that the first meant the inside of the Rink came with the second. . . ." In five additional cases the recognition did not appear until after the reaction.

From this it would seem that the image tends strongly to be at once upon its appearance either an *M* or an *I*. The ambiguity of these images is almost entirely a matter of interpretation based upon extraneous material. Except in the five cases in which the *I* appeared in a memory setting which was more or less a part of the image, and the one instance in which an *M* seemed to have imaginary details, this extraneous matter always came in *after* the appearance of the image as an *M* or *I*, and, if our introspective reports may be relied upon, it was almost invariably in the form of thought references which had no imaginal content.

It does not appear to be worth while to report here the behavior of these equivocal images with reference to all the various criteria which have been applied to the *M*'s and *I*'s. After a detailed study of these correlations, the writer has failed to find any facts which would either tend to alter our previous accounts, or supplement materially our knowledge concerning them. We shall therefore close the chapter of our results at this point.

IV. Summary and Conclusion

It has been the main object of this paper to consider statistically certain experimental criteria with reference to memory and imagination in a series of projected visual images. We may summarize our results as follows:

1. An attempt to study eye-movements with the two classes of images proved unsuccessful. In so far as any positive results were indicated, they do not appear to warrant the assumption of a correlation between eye-movement and memory images.

2. With regard to localization, we have found that both *M*'s and *I*'s may be located in all of the four groups which we have distinguished. There appears, however, to be a more or less general tendency to locate *M*'s at a distance, and *I*'s in the near-by space, usually upon the disk.

3. The presence and absence of color did not reveal any marked correlation with either class of images.

4. Affective states of any sort were very infrequent. When present, they usually attached themselves to memory-images.

5. *M*'s were reported to be distinct in a larger proportion of instances than *I*'s.

6. On the whole, spontaneous imagery was less frequent than non-spontaneous imagery, but the difference is not great. *M*'s appear to be more often spontaneous than *I*'s, although two of our observers mark an exception to this rule.

7. The average reaction-time for *M*'s is noticeably less than for *I*'s. In this the individual results agree.

8. Associations were found, in general, to be more frequent with *I*'s than with *M*'s. The *I*'s also tended to be more stable than the *M*'s, but were less often plastic.

9. A study of the equivocal images indicates that these images are, as a rule, either *M*'s or *I*'s upon appearance, the equivocal judgment being usually based upon extraneous matter.

From these results we are unable to conclude that our experimental criteria afford adequate grounds for a specific differentiation of memory and imagination. At best, as with reference to localization, distinctness and reaction-time, we can but speak of more or less general and habitual tendencies. The real distinction appears to elude any attempt to reduce it to such factors and conditions as we have been studying. We are therefore forced, it would seem, to refer the matter to imageless acts and contents which, for the most part, have

been without the range of our present investigation, and to agree with Külpe when he says: "A certain content becomes recollection by a judgment connected with it, and this judgment can be produced by extremely different causes. Imagination, in the same way, is characterized not by the appearance of particular series of sensations or ideas, but by the realization that the given ideas present something new, never before experienced in this form, but possibly to be perceived in the future."[1]

To trace the matter farther at this time, would require us to encroach upon that portion of our work which we have already mentioned as the special occasion for this series of experiments. So far as our data allow us to judge, however, memory and imagination appear to be *meanings*, and as such are not capable of complete reduction either to certain characteristic and invariable sensory and imaginal attributes, or to a certain contextual setting which may in turn be analyzed into terms of sensory and imaginal content. A full justification for this conclusion, however, we must defer until a more complete account of the meaning process is before us.

In conclusion, we shall content ourselves with a few remarks concerning three different modes for determining the meaning of an image as remembered or imagined, which are revealed in our introspective reports:

(1) A determinative tendency may be aroused by a thought or other content which precedes the appearance of the image. (2) A judgment may be formed after the appearance of the image on the basis of attendant thoughts or associated images. (3) The awareness of the category, memory or imagination, to which the image belongs, may be independent of any other contents, and quite implicit in the image itself. In the first mode, we have to do with determinative tendencies which issue, not from the instruction alone, but from the stimulus-word, or a thought which attaches to it. In the second mode we have a judgment which is more or less explicitly founded on a relationship established between the image and some attendant thought or image. The third mode deserves more extended consideration. We have already pointed out evidence of its

[1] *L. c.*, p. 188.

existence in our analysis of the equivocal images, the majority of which seemed to be *M*'s or *I*'s immediately upon appearance. But before this fact is accepted unreservedly, it should be subjected to a more detailed investigation. Should it resist all attempts to reduce it to either of the two modes previously mentioned, two alternate explanations suggest themselves. The first, and most familiar, one is the assumption that an affective-tone is attached to the image which distinguishes the known from the unknown. Our observers have frequently used the terms "familiarity" and "recognition" to indicate the fact that an image is remembered, and it is usual to consider this as evidence for the presence of affection. But our results afford no grounds for substantiating such a conclusion. Very often the observer reported an image as "familiar" or "strange," but could detect no feeling attached to the experience. From our other investigations, as well, we have failed to find that a judgment of familiarity was necessarily, or even usually, based upon an affective-tone. More often it appeared to be a purely rational procedure. We cannot, of course, attempt to decide this question here, and, as already pointed out in § 4, it is possible that many of the finer nuances of feeling which may have been operative at the time of their appearance, were neglected in the introspection. We are at least justified, however, in presenting the alternate view, which is also reached by Koffka,[1]—namely, that every image has its imageless parts, which are in so far independent that they may exist alone in consciousness. These imageless accompaniments would, therefore, be the elements by means of which it is possible to base a judgment which refers an image to the category of memory or imagination when neither feeling nor attendant thought, sensation or image has been operative.

We shall not press these tentative conclusions, but be content with their mere enumeration. It appears highly probable, however, that a searching investigation into the essential nature of *recognition* will be requisite before the real basis of the distinction between memory and imagination can be brought to light.

[1] *L. c.*, p. 365.

THE LAWS OF LEARNING[1]

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Modifiability of behavior has now been demonstrated in a large number of animals, ranging from the single-celled stentor and paramæcium up to the complex mammals and man. Two inferences lie near at hand: First, that modifiability of behavior is a general characteristic of animal protoplasm. Second, that the several processes which in various animals are called learning, habit-formation, memory, association, etc., are at bottom one and the same process and explainable by the same laws. As the phenomenon is demonstrated in one after another animal it becomes increasingly important to determine the primary laws upon which learning depends. It is the purpose of this paper to analyze certain factors involved in the simplest case of learning, to advance an hypothesis regarding the fundamental laws of all learning, and to show the value of this hypothesis in the explanation of different forms of learning.

One of the simplest cases of learning is to be found in the stentor described by Jennings. This behavior occurs when a large quantity of carmine grains are sifted into the water currents which the stentor draws toward its disk.

"Under these circumstances," says Jennings, "the normal movements are at first not changed. The particles of carmine are taken into the pouch and into the mouth, whence they pass into the internal protoplasm. If the cloud of particles is very dense or if it is accompanied by a slight chemical stimulus, as is usually the case with carmine grains, this behavior lasts but a short time; then a definite reaction supervenes. The animal bends to one side—always in the case of the stentor to the aboral side. It thus as a rule avoids the cloud of particles unless the latter is very large. This simple method turns out to be more effective in getting rid of stimuli of all sorts than might have been expected. If the first reaction is not successful, it is repeated one or more times. This reaction corresponds closely with the 'avoiding reaction' of free-swimming infusoria, and like the latter, is usually accompanied by revolution on its long axis,—the animal twisting on its stalk two or three times as it bends toward the aboral side.

[1] A first draft of this paper was read at the meeting of the Western Branch of the American Psychological Association, University of Chicago, April 5, 1912, under the title 'Transferred Associations.'

"If the repeated turning toward one side does not relieve the animal, so that the particles of carmine continue to come in a dense cloud, another reaction is tried. The ciliary movement is suddenly reversed in direction so that the particles against the disk and in the pouch are thrown off. The water current is driven away from the disk instead of toward it. This lasts but an instant; then the current is continued in the usual way. If the particles continue to come, the reversal is repeated two or three times in rapid succession. . . .

"If the stentor does not get rid of the stimulation in either of the ways just described, it contracts into its tube, In this way it, of course, escapes the stimulation completely, but at the expense of suspending its activity and losing all opportunity for food. The animal usually remains in the tube about half a minute, then extends. When its body has reached about two thirds its original length, the ciliary disk begins to unfold and the cilia to act, causing currents of water to reach the disk, as before.

"We have now," continues the author, "reached a specially interesting point in the experiment. Suppose that the water currents again bring the carmine grains. The stimulus and all the surroundings remain the same as they were at the beginning. Will the stentor behave as it did at the beginning? Will it at first not react, then bend to one side, then reverse the current, then contract, passing anew through the whole series of reactions? Or shall we find that it has become changed by the experiences it has passed through, so that it will now contract again into its tube as soon as stimulated?

"We find the latter to be the case. As soon as the carmine grains reach its disk, it at once contracts again. This may be repeated many times, as often as the particles come to the disk, for ten or fifteen minutes. Now the animal after each contraction stays a little longer in the tube than it did at first. Finally it ceases to extend, but contracts repeatedly and violently while still enclosed in its tube. In this way the attachment of its foot to the object is broken away and the animal is free. Now it leaves its tube and swims away."

I take this bit of behavior for what the experimenter interprets it to be, namely, a case of modification through experience, a case of learning, though one must admit that it is probably an easily forgotten bit of education. In trying to derive from it the primary laws of learning I shall first present Jennings' own explanation. That author's fundamental formula for the behavior of organisms is this: the external stimulus plus the present physiological state of the organism equals the reaction. In the behavior of the stentor just described the external stimulus remains the same; the behavior of the animal changes; therefore the stentor must have undergone a change in its physiological state. Symbolizing the physiological states accompanying the changes in reaction by capital letters we have in succession states *A*, *B*, *C*, and *D*. Jennings writes:

"The course of the changes in physiological states may then be represented as follows:

$$A \rightarrow B \rightarrow C \rightarrow D$$

Now we find that after many repetitions of the stimulation the organism contracts at once as soon as the carmine comes in contact with it. In other words, the first condition *A* passes at once to the condition *D* and this results in immediate contraction.

$$A \rightarrow D$$

It seems probable that the same series occurs as before, save that conditions *B* and *C* are now passed rapidly and in a modified way, so that they do not result in a reaction: but are resolved directly into *D*. The process would then be represented as follows:

$$A \rightarrow B' \rightarrow C' \rightarrow D$$

But whatever the intermediate conditions, it is clear that after state *A* has been resolved through pressure of external conditions into state *D* this resolution takes place more readily, occurring at once after the state *A* is reached."

In the last paragraph Jennings makes a gratuitous assumption which is probably not correct. He suggests that states *B* and *C* persist but in a 'modified way.' This is gratuitous, because there is no evidence that they do persist and from what we know about learning in other cases we are justified in inferring that they do not persist. In learning any bodily act what one experiences is the elimination of useless intermediary movements. If on his first attempt one makes a series of movements which may be designated as 1, 2, 3, 4, 5, 6, 7, 8, 9, he may on the second attempt drop out movement 3 and in succeeding practices lose all but Nos. 1, 6 and 9. When he has once acquired the 1–6–9 habit the intermediate steps have been suppressed and eliminated from the series. He does not experience acts 2, 3, 4, 5, 7, 8 as 'passing in rapid succession.' They simply are not there at all when the habit has once been perfected. In fact after the formation of many habits the injection of the intermediate steps becomes an impossibility. If one has difficulty in believing this let him try to walk, or write, or swing indian clubs or ride a bicycle, or do any other complicated act in exactly the manner in which he at first did it. All of us at one time talked in the halting child fashion. Not one in a thousand of us can do it now and it is only the exceptionally skillful professional reader who can create either for himself or others the illusion of child speech. Yet at one time it was the natural manner of every person. If these intermediate steps still persist but pass more rapidly and in a modified way why is it not possible merely by slowing down the process to reinstate each of these rapidly passing

states in the original series? What we all know is that slowing down the process does us no good whatever. The reasonable inference is then that the 'useless' intermediate states have been wholly eliminated. They have been suppressed and permanently inhibited, short circuited by virtue of originally distant states becoming fused together.

In interpreting the modification of the stentor's acts then it is fully as consonant with the observed facts of behavior in the stentor and far more in harmony with the facts of human learning to say that, in the modified behavior of the animal, states *B* and *C* have been wholly eliminated. They are no longer 'passed rapidly and in a modified way.' They are simply not there at all. Instead of having

$$A \rightarrow B' \rightarrow C' — D$$

we have

$$A \rightarrow D.$$

It is probably true as Jennings states his law of learning that "the resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times," but such a law will not account for the fact that at one time a physiological state *A* will 'be resolved' into another physiological state *B* and at another time the same state *A* will be resolved into state *D*. In order to account for this shift of 'resolution' another law has been proposed called the law of effect. This law says that there is something about the state *D* which determines that *A* shall be 'resolved' into state *D* instead of into state *B*. *D* has a reversionary influence and determines the course which the state *A* shall take on its next appearance.

Now there can be no doubt that the 'law of effect' furnishes a much needed supplementation to the 'law of exercise,' or the law of the 'readier resolution of physiological states after practice.' There is simply an unlimited number of cases, many of them experimentally determined, which demand both of these laws. However, when one tries to conceive what the law of effect can mean in detail and especially what it can mean in such a simple organism as the stentor or paramæcium

he strikes a number of very puzzling problems. One of these has already received consideration at the hands of numerous writers. This is the problem of the nature of the effect; and it must be admitted that the discussion has not as yet led to any very illuminating results. The recent discussions which have held positively to this theory have been content with merely affirming that 'effect' exists and leaves to further investigation the problem of determining the nature of the effect in question. It is the further purpose of this paper to examine some of the conditions under which 'effect' can operate. It makes no pretense of giving a complete analysis.

When one tries to conceive a series of physiological states there generally comes to mind a series of physical objects. One may think of a series of cars, a series of waves in the water, a series of sounds or a series of movements of an animal such as the successive steps in walking. In each of these cases the series is made up of two or more units more or less closely juxtaposed. Taking the series in which the separation is least distinct, namely the waves of water, you yet are able to distinguish a region within which you can say that one wave ends and the second begins. It is clear that the crest of one wave is gone from a certain point before the arrival of the second crest, and further, there is no conceivable connection between wave one and a third or fourth wave except through the intermediate waves. Now if one carries this conception over to the case of learning and applies it to the case of the stentor then he must understand that state *A* is directly connected with state *B* and through *B* with *C* and so on. There is no connection between state *A* and state *D*. But upon this hypothesis there is no conceivable way in which state *D* could become connected with state *A* in such a manner as to eliminate *B* and *C*. One must conceive that state *A* in some manner persists until state *D* comes into existence. It is only when the two states coexist that the law of effect can work. When they do coexist we can imagine that there is some condition about *D* which ties it to *A* rather than to any other state. But in order that we may have the coexistence we must have a condition in which a physiological state overlaps and reaches

beyond its immediately neighboring state. This condition I have chosen to call the *condition of irradiation* and to name a law which may be called the *law of irradiation.* Stated simply this law is that *a physiological state is not self contained but it tends to flow over and mingle with other physiological states and extends to states temporally removed from it.*

That two or more physiological states may coexist in a single celled animal is shown by the behavior of paramæcium under two or more stimuli. "If the animal is at rest against a mass of vegetable matter, or a bit of paper under the action of the contact stimulus, and is then struck with the tip of a glass rod, we find that at first it may not react to the latter stimulus at all. A touch that would cause the free swimming specimen to give the avoiding reaction in a pronounced way often has no effect on the quiet specimen. Sometimes, however, a touch coming from behind causes the animal to move forward, still remaining in contact with the solid object; it thus creeps a short distance over the surface of the solid. . . . Thus we find that under the simultaneous action of the two stimuli the infusorian may either react to the more effective of the two, whichever it is, without regard to the other, or its behavior may be a sort of compromise between the usual results of both." In such a case as this you have clear evidence that a single cell may be the bearer of two or more physiological states at the same time and that both may be effective at the same time in determining the animal's behavior. This fact gives us additional reason to believe that the appearance of a physiological state *B* in the series does not thrust out of the organism all other states and gives ground for believing that state *A* may still continue across several intervening states.

The facts thus evident in protozoa have their counterpart in the results of experiments on remote and retroactive memory in human beings. In these experiments a series of nonsense syllables is learned by the Ebbinghaus method. It is found then that if the series is turned about and learned in the reverse order there is a distinct saving in time in the second learning. Similarly it is found that if each alternate syllable is omitted and the remaining syllables are learned in the direct

order there is again a distinct saving over learning the same number of syllables without the previous learning. It is thus indicated that the associative ties formed in the first learning are formed not only between the successive members of the series but that associative connections are also made between the alternate members, and further that connections are made in the reverse direction from the original learning.

It is difficult to speak of the radiation of a physiological state without thinking of it in purely spatial terms. Yet in the case of the single celled animals there is no ground for assuming spatial factors. There are no organs spatially arranged which could mediate the facts observed. What we have sufficient ground for inferring is the changes in physiological states. The nature of the change we must refer to physiological chemistry if it is to have a scientific explanation at all. Just for this reason I would distinguish the radiation of a physiological state from the concept of diffusion applied to the nervous conditions in the complex nervous systems. The latter is always conceived in spatial terms and when one thinks of a nervous impulse diffusing through the nervous system he thinks of a fluid running through tubes or some other mechanical contrivance. It is just this conception which is not applicable to the cases in lower organisms and it may be due to this mechanical conception that we remain in such gloom about the relation of the brain to higher animal activities.

Such a theory as I have outlined has important bearing upon the psychological and neurological problems in the psychology of learning. Let us consider the neurological problem first. All our attempts to know what goes on in the brain during a learning process are founded upon a mechanical conception of the nervous system. We talk of cord and cortex, of neural levels and localization, of cells and fibers, of neurones and synapses. *Our problem is always to conceive how such a mechanical system can so operate that variable behavior is possible.* The latest attempt to provide an intelligible theory is to be found in the literature of the synapse. It is in the membranes between the fine tendril endings of the neurones we are told that there exists the condition for variability of behavior.

These membranes offer resistance to the passage of the nervous current and here something must occur which is different from the ordinary passage of the nerve impulse through a neurone. In one case the resistance may be overcome in one direction but at another time it is broken down in an entirely diverse direction. The cortical neurones are supposedly primitive cells and yet retain some of their original protoplasmic properties among which is the power of amœboid movement. A stimulated neurone stretches out one of its processes in one direction and touches or comes near the processes of a neighboring neurone and there is thus formed a physiologically continuous circuit. When once the impulse has run its course the amœboid process recoils. At another time a different process may be thrust out touching a different cell and thus provide on the occurrence of the second stimulation an entirely different form of reaction. *What is still lacking to make this theory satisfactory is some explanation why at one time the one amœboid movement occurs and at another time another takes place.* As yet there has been no sufficient solution of this problem.

In the presence of the failure of the synapse theory to give us a sufficient explanation of learning and modifiable behavior it may be proper to inquire if we are not expecting too much from the purely mechanical nature of the nervous system. Possibly the condition within a single cortical neurone itself is more complex than we have been assuming it to be. If we allow ourselves to think of the cerebral neurone as retaining the characteristics of primitive animal protoplasm may it not be true that it retains the property found in the stentor and paramæcium of being the bearer of several physiological states at one and the same time. This is no more a speculation than it is to suppose that it still has the power of amœboid movement. There is reason to think that it may be a much more fruitful hypothesis. *Under this hypothesis the problem of learning becomes the problem of understanding the nature of intra-neuronic activity rather than that of inter-neuronic relations.* The crux of the learning problem shifts to a consideration of the origin and relation of the several physiological states occur-

ring simultaneously within single neurones rather than to a study of membranes and anatomically distinct elements. Structural neurology is, to a large extent, relieved of a task which it has not been able to meet and we are thrown back upon physiological chemistry and the study of animal behavior for our explanation.

It is in this last fact that the hopefulness of this theory inheres. If the behavior student of the lower organisms can give us an experimental analysis of all the factors involved in the learning of the stentor and paramæcium he may take us a long way toward explaining the neurological problem in human learning. Because the materials are so readily accessible there is some hope that he may do so.

The complete analysis of learning in any animal should greatly clarify our perception of the essential factors in all learning. Much of the discussion of this topic assumes that we have three sharply divided kinds of learning processes, namely, learning by trial and accidental success, learning by imitation, and learning by deliberation, by thought, or by reason. Whether or not these three forms of learning occur in either animals or man is a question to be determined by observation and experiment. But in attempting to experiment in this field the investigator confronts the difficulty of being unable to observe the learning process in detail. In the case of animals he is confined to such observations as he can make on the overt behavior of the animal and in the case of man he can only supplement this sort of evidence with introspective accounts of the learning process as it appears to consciousness. As a result there is always the possibility that the facts open to common observation may be but the outward expression of more recondite processes which can be only indirectly inferred. If we could only make a detailed and final observation on these hidden processes we might discover that they are common to all of the customary groups of learning. It may be that these several forms of learning are like the lines diverging from a single point. If one looks at them at some remove from their origin they seem far apart, but if they are traced back to their beginning it is found that they come from a common source.

Imitation as a finished product may seem very unlike trial and accidental success and the latter may seem much more unlike learning by deliberation, but if we could only get at the details of these processes they might be found to be very much alike and to be but variations of a common neural activity. Traced back to their common ground it might be found that learning by trial and accidental success, learning by imitation and learning by deliberation or by free ideas are all essentially like the learning of the stentor and that each of these forms of learning involves the law of exercise, the law of effect and the law of irradiation of physiological states together with whatever other laws the experimental analysis of animal behavior may bring to light.

In suggesting the discovery of yet other laws it is not intended that these new laws will invalidate the laws already worked out. Further progress would rather seem to lie in the direction of the elucidation and amplification of these fundamental laws already discovered.

In order to make clear what is meant by tracing the higher forms of learning back to the simpler forms let me illustrate by the case of imitation. A group of children were playing together on the floor. Suddenly one of the older children grabbed a cap from the chair and placing it on the end of her finger waved it about in the air above her head. A small boy three years old picked up another cap and placing it on the end of his finger waved it above his head. A naïve observer would have called this a case of imitation and for purposes of description there is probably no better way to designate the act in question. That the act was new to the second child was evident from the irregular manner of his movements and this was confirmed by the testimony of the parents who had observed the boy's development with critical interest. It was probably true that the particular combination of movements involved in the act was essentially a new experience for him. The stimulus for the new act was the behavior of the other child; the result was behavior much like the behavior of the child seen. This combination of circumstances, an act in one person serving as a stimulus to produce in another person behavior

resembling the behavior of the first person, constitutes the phenomena called imitation. Let us see now to what extent this imitative behavior can be explained by the elemental laws of learning stated above.

First it is to be noted that the imitative act is not wholly new. The child already has the habit of lifting his arm, clenching his fist, pointing his index finger, etc. There are also present the habits of observation, of seeing, etc., involved in perceiving what the other child does. It will simplify our explanation somewhat if we designate these two sets of activities by letters. Let the set of perceptive processes be designated by the letters, *a*, *b*, *c*, *d*, *e*, *f*, and *g* and let the muscular movements be represented by the letters *h*, *i*, *j*, *k*, *l*, *m*, and *n*. Now it is perfectly conceivable that these two habit chains might exist simultaneously in the nervous system and yet have no relation with each other that would be expressed in outward behavior. In such a case the observation of the playmate would never cause the child to go through the series of acts witnessed. There would be no imitation. That there are coexisting such unassociated habit chains in every nervous system there can be no doubt. But on the other hand suppose that the physiological state represented by *g* exists in close proximity to the physiological state *h* of the other habit chain and suppose that there was radiation laterally as well as in the direction of the temporal sequence, it is possible that something might cause state *g* to become coalescent with state *h* and all the rest would follow as a matter of habit. The only thing necessary to bring about imitation would be the possibility of radiation in a direction other than that of temporal sequence. In the formation of a habit a physiological state must overlap to the succeeding state of the series. In the case of imitative learning the overlapping must be to states not in the temporal series. If this possibility is granted all that is further needed is some variable that will for the first time cause this overlapping to be sufficient. This variable may be found in the states accompanying excitement, emotion, clearness of perception, etc.

The advantage of such a conception of learning as this is

that one can hold to a physico-chemical theory of animal behavior and yet not deny the existence of such activities as imitative learning, learning by free ideas, transfer of training, etc. Or to put it the other way he can recognize the existence of these latter processes and yet not have to invoke supernatural entities in order to explain them. They are all finally reducible to the same basic processes.

Take the case of what are called free ideas. A free idea is not an unrelated idea. It is merely a physiological state that is not rigidly bound within a single linear series of physiological states. Its lateral overlappings are as numerous and as potent as the linear ones, and they are not overcome and inhibited by virtue of its once being caught up in a particular chain of experiences. From this point of view the so-called free idea appears as of the same stuff as the simple physiological state of a stentor and a paramæcium. Between the condition of the infusorian and that of man there are many intermediate levels of physiological states, but all of them are of essentially the same sort of protoplasmic function.

In the light of all these matters the law of radiation may be stated as follows: A physiological state is not self-contained but tends to radiate to other physiological states both those which form with it a series of states like a habit chain and also to other physiological states which have never formed a series with it.

By virtue of this linear radiation the formation of single habit chains can be explained. By virtue of the lateral radiation can be explained all the higher forms of learning including imitation and learning by the use of free ideas. By virtue of the law as a whole all learning is reduced to physical terms and there is no longer need to invoke ghosts in the form of consciousness to explain the higher forms of mental activities.

DISCUSSION: IDEO-MOTOR ACTION

Dear Mr. Editor: In the preparation of an article on which I chance to be engaged, I have stumbled (more or less accidentally) on what I suspect is the solution of the obvious *cul-de-sac* in which Thorndike's 'Ideomotor Action' thesis cannily places many of us, and, ultimately, himself. We need not, perhaps, after all, instantly give up ideomotor action! Let us see.

As so often in the endless history of philosophic and scientific discussion, *most* of the difference in opinion seems to rest on an indefiniteness of definition of the essential concepts,—here "idea."

There are ideas and ideas—and more ideas. For example, one of us might be thinking an idea of throwing-a-javelin-straight-at-the-heart-of-his-enemy in an indolent, dim, ghostly way (vague or definite it matters not, nor whether visual, kinesthetic, or tactile), a mere idea, a memory image second- or third-hand, or twentieth-hand, of a savage about to hurl a javelin at another savage—a true Herbartian idea of an idea. Not a nervous kinetic strain concerned in such a conceptualization came from outside the neopallium perhaps. Women devotees to 'society' are apt to have this intensity of ideas for example about the life of shop-girls—or even about a man throwing a javelin. Ideomotor action under such adynamic motives certainly does *not* tend to occur; even if the lady had a javelin in her hand, she probably would not throw it, even though, again, she saw her bitterest enemy balancing up the front steps.

Now to my mind, Mr. Editor, this is not properly an idea of a movement at all; as has been already suggested, it seems more like the shadow of the ghost of a dream of a motor idea.

But on the other hand an intelligent savage who had killed already a number of his enemies by hurling javelins at their hearts might have an idea of this blood-curdling bit of human behavior that would be a real motor idea, conceived and born of immediate human experience, and tingling with flesh-and-blood *energy*, intense and murderous. Can we, any of us, doubt that *such* an idea would, if required, aid a repetition of the movement, bringing as it would to the innervation every necessary element of vigorous effort? But both of these 'ideas' are ideas.

There's another way of looking at it, not less real but more

technical. The difference between an habitual voluntary action (made up of familiar and hence more or less reflex movements) and a really voluntary action, new and personal and difficult, is much more important than most psychophysiologists yet realize. An habitual movement would be interfered with of course by germane ideas, by conscious attention—'local consciousness' as the voice-teachers term it. Throwing a javelin, however uncommon the actual implement or the act itself, is made up of habitual elementary movements. Undoubtedly then the idea of the action would interfere with it, and the more real it was, the more kinesthetic (until real skill had been developed), the more it would interfere. On the other hand, a mere cue, such as even a waxen image of your enemy might well supply, would tend not to interfere but would, on the contrary, tend to induce the action.

Given, however, a true, real, exact, living motor idea, tingling with feeling (innervated "upward" from the thalamus through the stimulus of the actuating kinesthesia) with *effort* added and implicit in the idea, *such* an idea would be, I take it, granted by that same 'overwhelming majority' of the members of the Association, to be an almost irresistible forerunner of action. We need not over-analyze, but we all do need to realize the dynamic nature of every idea that is quick and not dead, a reality and not a wraith. Plato is dead.

With thanks for your patience,

GEORGE V. N. DEARBORN

CAMBRIDGE,
April 8, 1913

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